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Oceanography, Population Resources and the World

Roger Randall Dougan Revelle
Preparation for a Scientific Career



Interviews Conducted by

Sarah L. Sharp

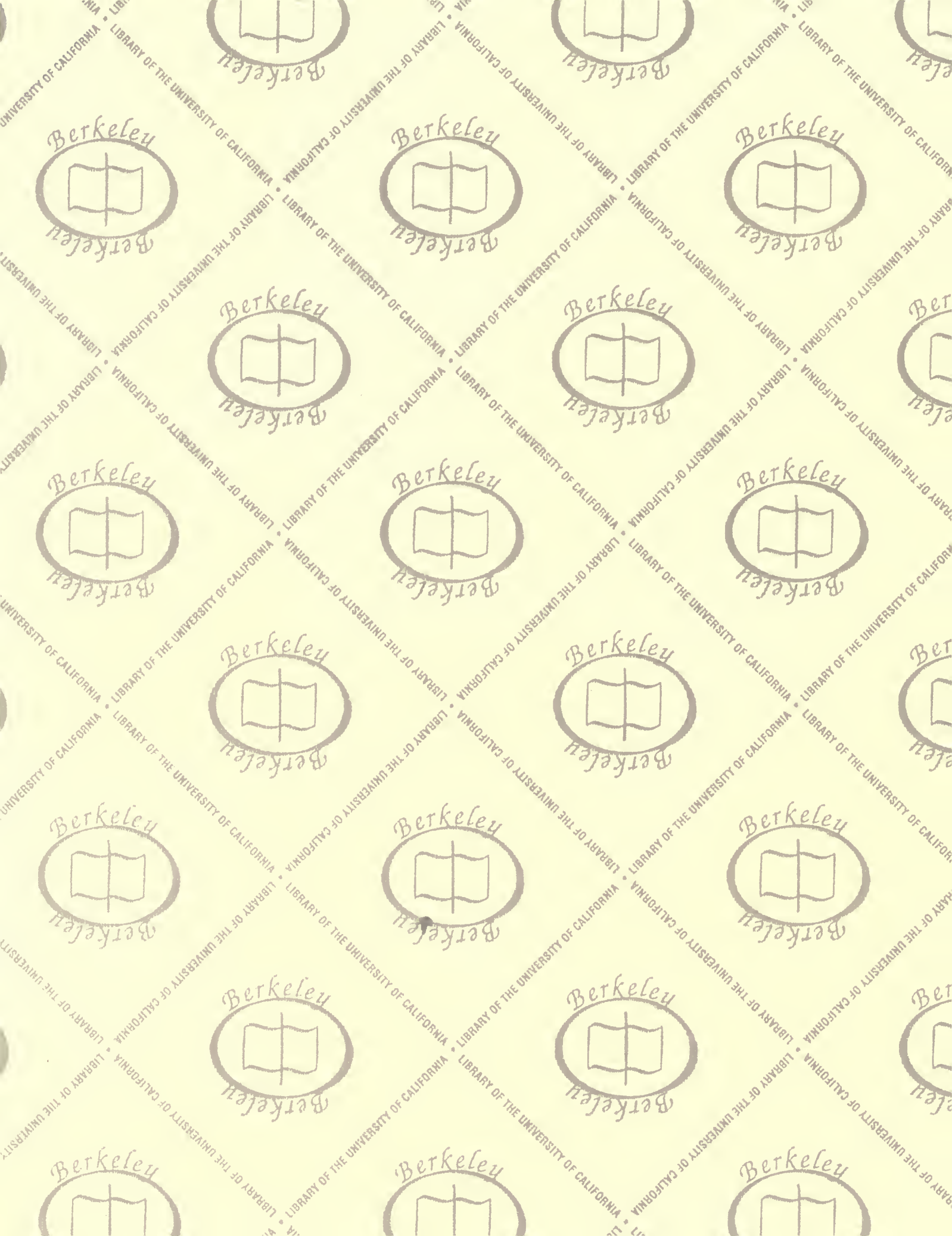
in 1984

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Roger Randall Dougan Revelle

PREPARATION FOR A SCIENTIFIC CAREER

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Sarah L. Sharp

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Bowling Green State University
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Berkeley, California
Regional Oral History Office

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SIO Reference Series No. 88-19. November 1988.

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Background of the Regional Oral History Office

Oral history is a modern research technique for preserving knowledge of significant events as recounted by participants. These tape recorded conversations are the vivid, irreplaceable view of a narrator who has been deeply involved in the events described, with the dynamic quality of the ancient oral tradition. Because it is primary material, oral history is not intended to present a verified, complete report. As a basic document itself, it is used to illuminate other, more conventional sources. These memoirs can also inform current leaders of the thinking and practices of their predecessors.

An oral history memoir is a recorded and transcribed series of interviews carefully designed to cover the major stages and events in the life and work of the selected individual to convey the uniqueness of his or her personality as well as contributions to important facets of California affairs. An oral history study is a set of interviews of varying length by a number of individuals who have observed the same aspect of human endeavor from varying viewpoints. Memoirists review their transcripts after editing by the interviewer. Transcripts are then retyped, indexed, bound with photographs and illustrative materials, and placed in The Bancroft Library and other suitable locations. The memoirist receives a copy for his or her own use. The Bancroft Library safeguards and administers the use of the memoir, and other personal or business papers which may be donated, according to the narrator's wishes.

The Regional Oral History Office is almost unique in the field in maintaining a permanent staff of experienced interviewers, each knowledgeable about oral history techniques and also familiar with several aspects of the socio-economic, scientific, cultural, and governmental life of California. Each memoir is assigned to a specific staff person who follows through from initial research and planning of interviews with the narrator to the presentation of the completed volume.

There are at present six staff interviewers, with several others on call in more specialized fields. The Office is under the direction of Willa Klug Baum, herself an experienced interviewer-editor with a distinguished national reputation. Faculty members in departments concerned with given project fields are consulted and upon occasion undertake portions of interviewing. An interviewer normally works on only two or three projects at a time, in order to maintain close contact with memoirists. A full-length biographical memoir, or a set of shorter interviews on a single topic, requires on the order of two years to complete.

Although the University provides a modest budget for basic administration, the substantive work of the Office is funded by gifts and grants, which are tax-deductible. Funds are sought to undertake specific projects, developed by the staff and its faculty advisors, which are designed to broaden and enrich available materials on the significant factors in the life of the Bay Area, many of which have statewide and national implications. These project germinate from The Bancroft Library's continuing list of outstanding individuals and organizations whose accomplishments should be documented for posterity.

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On behalf of future scholars, the Regional Oral History office wishes to thank those who have generously donated funds to complete this oral history memoir project with Roger R. Revelle.

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Oceanography, Population Resources and the World

PREPARATION FOR A SCIENTIFIC CAREER

OBSERVATIONS ON THE OFFICE OF NAVAL
RESEARCH AND INTERNATIONAL
SCIENCE, 1945-1960

DIRECTOR OF SCRIPPS INSTITUTION
OF OCEANOGRAPHY, 1951-1964

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INTERVIEW HISTORY

Roger Revelle was director of Scripps Institution of Oceanography in La Jolla, California between 1951 and 1964, a period of intense development and expansion at this institution. Although we tend to think of individuals in this type of position as forever adults and fully trained, this volume allows the reader to reflect on Revelle's childhood, early schooling and graduate education with him, and sense his "preparation for a scientific career."

Transcripts of two oral history interviews, conducted on 26 and 27 January 1984 with Dr. Revelle, and edited both by the interviewer and the interviewee, are included in this volume. Revelle reminisces about his early years spent in Seattle, Washington and Pasadena, California. Later education followed, taking Revelle to Great Britain and Norway, experiences which perhaps opened the way to his interests in international scientific cooperation. These recollections take a somber turn with the interviewee's comments on his oceanographic work in the navy during World War II and his role as head of the American scientific team which conducted research on various oceanographic effects of atomic bombs dropped as part of Tests Able and Baker during Operation Crossroads in 1946 at Bikini Atoll in the Marshall Islands.

In order to prepare sufficiently for these interviews, the interviewer-editor conducted research on several levels: examination of the Roger Randall Dougan Revelle Papers which have been collected at the SIO Archives in La Jolla; reading of secondary works which highlight the recent history of oceanography and other areas of Dr. Revelle's career and life; and, consultation with Dr. Revelle himself about critical episodes which he thought needed oral documentation.

The significant contributions to oceanography which Dr. Revelle has made came to the attention of the Regional Oral History Office through Professor Harry N. Scheiber of the Law and Society Program at Boalt Hall School of Law at the University of California, Berkeley. Professor Scheiber was instrumental in the interviewer-editor's obtaining a seed grant from the UCSD Chancellor's Office to initiate preliminary research and interviewing on this oral history project.

Sarah Lee Sharp, Ph.D.
Project Director
Interviewer-Editor

Fall 1988
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Bowling Green State University
Bowling Green, Ohio 43403

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I FAMILY, BOYHOOD AND EDUCATION

[26 January 1984]##

- Sharp: I thought we might just start by talking a bit about your family and about growing up. So, the first part of today will be very biographical, not involved in science too much at all, but rather the earliest parts of your life.
- Revelle: Shall I just talk then?
- Sharp: Yes.
- Revelle: Well, I was born in Seattle, on March 7, 1909, and for the first six or seven years of my life we lived in Seattle in a house near the eastern shore of Lake Union. My father was a lawyer in Seattle. He was a member of a law firm with his two brothers, called Revelle, Revelle & Revelle. My mother was the daughter of an early Seattle settler, a man named James Dougan, who migrated from County Down in Ireland, in Ulster, Northern Ireland. Dougan started out as a carpenter in Seattle, and then became, as good red-blooded Americans do, a real estate man. He developed sub-divisions, or at least a sub-division, out near Greenlake.
- Sharp: Was your father Irish?

##This symbol indicates that a tape or a segment of a tape has begun or ended. For a guide to the tapes see page 82.

Revelle: No, that was my mother's father. My father came from an old family on the eastern shore of Maryland. His ancestor, Randall Revelle, settled in Somerset County, which is the southern-most county on the eastern shore of Maryland, sometime around 1640. There is some reason to believe that he was actually in the free colony, the Calvert proprietorship of Maryland, by about 1620, although that seems awfully early. He came from England; he was an Anglican.

My father thought that the family had originally been Huguenot, from southern France near Toulouse. There's a town near Toulouse called Revel. It could very well be that that's the origin of this particular branch of the family.

But there are quite a few Revelles or Revels or Revells in England.

There are at least three different Revelle families in the United States that are not really related to each other, as far as I know. One was a Chicago merchant, Alexander H. Revelle, who ran a furniture store in Chicago. Another was Fleming H. Revelle, publisher of religious books. The third was this family that lived on the eastern shore of Maryland.

I don't really know much about what happened between the time of Randall Revelle and my grandfather, except that the family came down a good deal in the economic scale. My grandfather was a farmer. He farmed about 160 acres on the eastern shore, and he was also an oyster fisherman. He was called Captain George Revelle because he had a Chesapeake Bay bug eye, which is one of those clipper-bowed, rake-masted boats they use for oyster fishing on Chesapeake Bay.

My father told me that my grandfather caught about three thousand barrels of oysters a year, which he sold for \$1 a barrel. A barrel of oysters is not very big, it's only about so big [measures out]--about a foot high and seven or eight inches in diameter. With that cash income he sent four or five of his children to college. They went to Western Maryland College, in Westminster, Maryland. There were his two brothers, George and Tom, and my father, whose name was William Roger. I think there was one sister, Mary Revelle--Aunt Maimie she was called.

My grandfather, Captain George, was married twice, and he had something like nineteen children between his two wives. My father didn't really know his half-brothers and sisters by the first wife very well. There were eleven in my grandmother's (the second wife) family, of whom seven survived. They had malaria--they used to call it chills and fever--there on the

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Revelle: eastern shore. One way or the other only about seven out of the eleven grew up. I remember one was named Joe Revelle; he became a policeman in Seattle.

There were two daughters, Aunt Mary or Maimie, and Carrie, who took care of their mother in later life in Seattle, Aunt Carrie. My grandmother had serious arthritis, rheumatism they called it then. She was confined to a wheelchair. Then there were five brothers, Tom, George, and William Roger, Joe the policeman, and a man named Chevalier de Tornay, who became a store keeper in West Virginia.

So they all moved away from the eastern shore, every one of them, after 250 years of peasant life in a place called Fairmont, which is near Princess Anne.

I think my ancestor Randall Revelle was the founder of Princess Anne, or at least one of the very early builders there. It's a quaint old town. Hardly a building has been built there since the eighteenth century. It's a very backward part of the country nowadays. Sort of a Maryland tobacco road. There are lots of Revelles still there.

We visited there once or twice, in Somerset County, when we were living in Washington. It was a very disappointing experience because they didn't give a damn about me or us, and they didn't really seem to care much about anything; they all seemed to have mangy dogs and hookworm. And the fishing, of course, has gone down a lot, that's one of the reasons for the decline. Even so, it's still a great water country. They call them watermen, the fishermen. The capital of that fishing industry, which is mostly for crabs now, is a place called Crisfield.

My father was quite bright; he was the valedictorian of his class at Western Maryland College; it's a little college in Westminster, Maryland, in the western side of Maryland. Later, he was head of the preparatory school, which was typical of colleges in those days--they had to train their own undergraduates. They had to educate their own undergraduates in high school first, so they could be eligible for college.

He then went across the country to Seattle. I think his older brother, Tom, who was a Methodist minister, had preceded him. And they all three studied law at the University of Washington.

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Revelle: In later life I remember my Uncle Tom was one of the most profane men I've ever known. [laughs] He was an eloquent man, as Methodist ministers were in those days, I guess, and he later became the United States attorney for western Washington under the Harding-Coolidge administrations.

Sharp: Was it just a general law firm that they had?

Revelle: Yes. They didn't specialize. It was a typical western American law firm. Their offices were in the New York block in Seattle, which has now been torn down. I think it has been replaced by something called the Dexter Horton building.

My mother went to the University of Washington. Her name was Ella Robena Dougan. They met there, I guess, at the University of Washington and got married some time in the early nineteen hundreds.

My father was born in the 1870s. I'm not quite sure when-- we could find that out. He was interested to some extent in the genealogy of the family and he wrote a little paper about it which we have a copy of, in which he stated what I've been telling you about his ideas about where they began.

Randall Revelle, the first Revelle in our branch of the family to come to this country, was a cooper. A cooper means a maker of barrels. The reason that he came to the colonies was not to get away from religious persecution--he was a good Anglican--it was just because he was tired of making barrels, as far as I can make out. And he became quite prominent. He was one of three commissioners of Somerset County, in other words, the people who were given the responsibility by the Calverts, the proprietors of the Maryland colony, to settle and subdivide Somerset County.

He had a brother-in-law named Edmond-Scarborough. Scarborough was a Virginian, and Virginia was moving up from the south. The southernmost county on the eastern shore was part of Virginia, Somerset County was the southernmost Maryland county. The Calverts became suspicious that Randall Revelle was a fifth columnist for his brother-in-law Scarborough, so they fired him as a commissioner.

There were two other commissioners, one was a man named Elzey. Randall Revelle built a big house on one side of the Minokin River in Somerset County, and Elzey built a big house on the other side. These houses are still standing. Revelle's house was called Clifton. It's been so modified now that it

Revelle: isn't very much of interest historically, whereas Elzey's house is still pretty much a colonial house. Clifton now belongs to one of my distant cousins, Mrs. Carpenter, who was married to a DuPont. This DuPont relative, whose last name was Carpenter was president of the DuPont Company at one time. I corresponded with her about thirty years ago, and she might not be alive anymore, but the house is still in her family.

Sharp: What particular religious influences did you have?

Revelle: Very little.

Sharp: The Methodist--.

Revelle: No. I'll come to that in a minute. My parents had another child, a daughter named Eleanor, who was three years younger than I was, born in 1912. And my mother developed tuberculosis. She came down here to southern California for her health, stayed a year or so as I remember. My father and my sister and I stayed in Seattle. Then we moved down here in about 1916 or 1917--it must have been 1916, because I remember going to the San Diego fair, the Panama Pacific Exposition here in San Diego. I remember particularly that we rented one of those electric carts (an early version of a golf cart), which my father was driving, and he couldn't drive it very well and ran into a curb and I bounced out of the cart on my head.

Sharp: Were you hurt?

Revelle: No, I wasn't hurt. I guess I had a hard head. [laughs] But that's one of the mental pictures I have of my early childhood. All you really have by my time of life is just mental images of isolated events.

I remember breaking my arm in Seattle, and having it x-rayed in the hospital. What I remember about that was that it was a complicated piece of machinery with lots of wheels. Nothing like the streamlined x-ray machines they have now.

I remember my father bought me a balloon, and one day he went off to his office and I was playing with this balloon. It popped and broke. I didn't dare tell him that I'd broken it, so I said it got away from me and went over the hill. He at least pretended to believe this. This was the first lie I ever told. I must have been about the age of three or so. I still remember it quite vividly.

Revelle: I remember seeing my mother and the new baby in the hospital in Seattle.

I remember also boats on Lake Union which sometimes turned over and people either drowned or didn't drown, as the case may be. Those were always dramatic occasions.

Anyhow, in 1916 we moved to California because of my mother's health. She'd gotten over the tuberculosis but she had asthma, and had asthma the rest of her life.

Sharp: Was Pasadena picked because that was--.

Revelle: We first went to Hermosa Beach. I don't know why we went to Pasadena. We lived in one of those little typical southern California beach houses. All the beaches in those days had something called a speedway, which was a narrow sort of an alley back of the first row of houses along the beach and before the second row, between the sea and the main road. You can see one like that here on Mission Bay now, the speedway. Hermosa may still have one.

Sharp: Do you remember the move at all, remember coming down and people packing things up?

Revelle: No, I don't. I do think I remember, though, that we came down on a boat, a boat called The Congress, which like most boats in those days eventually burned and sank. I think that was how we got here. But anyhow our first house was at Hermosa Beach. Then we moved to Alhambra, and lived there for a while. Then we moved to Pasadena, really to Altadena, 1451 North Holliston Street. North of Washington Street, which is really part of Altadena.

Sharp: Did your father then just go in with a new firm?

Revelle: No, he never really re-established himself as a lawyer. He must have been at least fifty years old when we moved down here. He went into teaching. He taught at the John Muir School in Pasadena and later at John Marshall School in Pasadena--taught social science. He also gradually built up his law practice again, but very slowly, and never really made a living out of it, I don't think, not after he moved down here.

Sharp: But he did that pretty much by himself? Didn't go into a firm.

Revelle: Yes, that's right. It must have been quite a traumatic experience for him to move down here. He was a very gentle, nice man, a very sweet-tempered person. Because of my mother's asthma and

Revelle: her general invalidism, he did most of the housework, including the cooking. He made bread, I remember, once a week.

Sharp: Did you get to help him at all?

Revelle: A little bit.

My mother died in 1938. He and she had gone on a trip to Seattle. Her mother was still living in Seattle--my mother's mother. This was Mary Dougan. Her name was Holmes before she was married. She was a Canadian from New Caledonia, Ontario. I think there are a few Holmes's still there. And my mother told me that they were related to the Oliver Wendell Holmes family in Massachusetts. She had long survived her husband, James, and long survived my mother.

My mother got typhoid fever on this trip to Seattle, probably from eating some infected lettuce in a salad, came back here to California and went into the Huntington Hospital for several weeks and finally died. She must have been, by that time, around 55 years old. She graduated from college in 1902 or 1903, which was 35 or 36 years before. She must have been about 22 when she graduated. That would be 57 or 58 wouldn't it?

Sharp: Yes, yes.

Revelle: Anyhow, she was in her middle or late 50s. I remember one thing about that occasion, and that was that they gave me a very heavy shot of typhoid--of the infectious typhoid organism so to give me a mild case of typhoid so they could take blood from me and give it to her, try to immunize her. So I've never been afraid of having typhoid since then. I never have a shot anymore. But it didn't do any good, she died. The doctor said that it probably was a good thing that she did because she never would have recovered very well. My father after a while married again.

Sharp: I have seen a note that he had written and her name was Mildred?

Revelle: Mildred, yes. I don't remember her name before she married him. She'd been married before.

Sharp: Didn't she have some children?

Revelle: No, she never had any children. She was a teacher, a very, very nice person, much easier to get along with than my mother.

Revelle: Mother was a very nervous, worrying person. And also, as I said, an invalid; she'd have asthma attacks at night very often. I would get up and rub her back, in the middle of the night--that seemed to give her some relief. But it's a terrible affliction, asthma. She was always very thin. I don't think my mother and father had much of a sex life, at least after they moved down here. They lived in separate bedrooms.

Sharp: Did her health not being so good, and your father having to do quite a bit of the housework and the cooking, did that mean that you spent perhaps more time amusing yourself or not so much time with them in terms of visiting or playing--?

Revelle: No, my mother had very peculiar ideas about bringing up children. I didn't go to school until I was eight years old.

Sharp: Did she teach you at home?

Revelle: Well, I taught myself. She taught me some but mostly I taught myself. I learned to read at a very early age, but I never learned to write. I read, I think, everything in the Book of Knowledge that we had at home by the time I was eight years old--twenty volumes.

Sharp: Just all day long, in the evenings or just anytime?

Revelle: Most of the time. I didn't play much with other children. I didn't know how to play with other children very well. We had some friends--two girls, two sisters who lived just back of us in Chester Street, the Tompkins girls, Jean Tompkins and Grace Tompkins. We played with them quite a bit and my sister played with us, but since she was younger she had a rather hard time of it. We also had a friend on Chester Street named Dick McCurdy. He moved in somewhat later, though, I guess.

When I first went to school at the age of eight, I went to the Henry Wadsworth Longfellow Grammar School on Washington Street about 6 blocks west of Holliston. I didn't have any idea how to get along with other children, and I used to get fairly badly beaten up the first year or so. Not really beaten up, but at least terrorized.

Sharp: Hard time?

Revelle: Yes. One of the things I remember very well was that we had a penmanship lesson, writing, and I didn't have any idea what the letters meant, so I just copied it like you'd copy a drawing. Of course it was pretty awful. I've never really learned to have very good penmanship since. One summer my mother sent me to summer school to learn penmanship.

Sharp: It didn't work?

Revelle: Well, you know, they had something called the Spencer method, with arm movement. Nobody uses arm movement. [laughs] It's one of those curious fads of the 1910 decade. So my penmanship, my writing, I can hardly read myself, and it's getting worse, if anything.

I started in the third grade at the age of 8 and was skipped from one grade to the next. I ended up in the sixth grade because I could do everything but write pretty well, academically that is. I was never athletic at all--very poor in athletics.

Sharp: Did you like going to school, as opposed to being more on your own?

Revelle: Oh, yes, I liked it very much. But I was always a little bit queer. I don't mean queer in the sexual sense, but in grammar school or high school I never was very popular or very good with girls or anything like that.

Sharp: Did your sister have the same experience?

Revelle: Worse than mine. She turned out to be quite tall, about six feet tall, which was much too tall for a girl in those days. She also had quite a bad complexion, acne. She had quite a hard time in grammar school and high school. I went to high school at the age of twelve, Pasadena High School, which was very young and my mother insisted that I wear short pants, knickers.

Sharp: Deadly.

Revelle: Yes, it was pretty awful. I remember one of the teachers in Henry Wadsworth Longfellow Grammar School was a Mrs. Seymour, and I remember the principal was a guy named George Hetzel, an austere, distant figure. Mrs. Seymour was the vice-principal and she was a good teacher. One traumatic experience I remember was that we were having class singing, which may be one of the things they still do, and Mrs. Seymour could hear there was something wrong.

Sharp: One person wasn't--.

Revelle: That's right. [laughs] She walked up and down the aisle listening to us all sing. Finally she stopped by me and said, "Why don't you stop singing and listen to the others for a while."

Sharp: Did you do that?

Revelle: Yes. But I still like to sing, and I can't sing any better than I did then. I at least like the idea of the sound. I can't carry a tune for sour apples.

Sharp: But you really like music?

Revelle: Yes, particularly classical music.

I remember three teachers in high school too. One was a Mrs. Magnusson, who taught algebra, and one was Mr. Van Amringe who taught science, chemistry particularly. I learned quite a bit from both of them, and liked them very much. There was also a Mrs. Peters, who taught public speaking. I remember I was on the debating team, or at least close to being on the debating team.

My wife, Ellen, went to Pasadena High School also. I never knew her there. She was two years behind me, and she was very shy. Ellen was quite socially popular. She was a member of something called the T-club in high school which was a rich girls' club and as a result of that she got elected to the Public Speaking Society. Mrs. Peters threatened to resign, because Ellen was such a poor, oh, almost completely impossible as a public speaker in those days. [laughs]

Sharp: So you would have had some of the same teachers?

Revelle: Yes. Sure.

Pasadena High School is now Pasadena City College. They may have several high schools in Pasadena now. There was just one then.

I remember the place across the street where we used to get milk shakes--all the kids hung around there a good deal--and waffles, wonderful waffles, I remember those, too.

I didn't do awfully well in high school, so I really had decided that I would go to Pasadena Junior College for a while, after graduating from high school, but I was one of Terman's gifted children. Did you ever hear of them?

Sharp: No, no.

Revelle: Well, there was a man named Louis Terman at Stanford, a professor of psychology there--he was the inventor of something called the Stanford-Binet Intelligence Test. When we were in grammar school, practically all the children in California were given this intelligence test.##

Revelle: He set up a long-term, what's called a longitudinal experiment, to follow a group of a thousand of the brightest children throughout their lives. They still follow the ones that are still alive. [laughs]

At the end of the summer after I graduated from high school, at the very last minute, while in fact it was opening, my friend Karl Rodi, whom I'd grown up with in Pasadena and who had gotten admitted to Pomona said, "Why don't you try going to Pomona? See if you can get in." My mother and I went out there, drove out there and she told them about my being one of these gifted children.

So I did get in, at the age of sixteen. And I did all right in college. Didn't become a member of Phi Beta Kappa--in fact I damn near flunked out during my junior year, but I became very much under the influence of a great professor named Alfred Woodford, a geology professor.

Sharp: You told me about that, but you said you'd originally thought about going into journalism?

Revell: That's right, I did.

Sharp: Did you think you wanted to do newspaper work or something like that?

Revelle: Yes. I was on the student paper in high school. I intended to go into journalism but then when I took this course in elementary geology from Woody, I decided I wanted to be a geologist. Really, it was entirely his influence. [telephone interruption] ## At Pomona, the reason I nearly flunked out was that in my junior year I was the co-editor of the Student Life, which we were trying to turn into a daily newspaper from a weekly newspaper. The man who tried to do this was named Jim White, and it was just too much for him. He had essentially a nervous breakdown. One of my fraternity brothers, Murray Putnam, and I took it over. I was at the same time taking physics and calculus. The physics class was at seven-thirty or eight o'clock in the morning and we used to stay up until about three every night, trying to get this goddamned paper out. [laughs] It was an impossible job really. It has now reverted again to a weekly newspaper, years ago.

Sharp: Sounds like a good way to go.

Revelle: But that was the year that Woody was on sabbatical leave. The physics professor was so disgusted with me that he wanted to flunk me out of college. Woody came back at the end of the year and just literally raised the roof with him. He hardly ever spoke to him after that; he was so mad at him.

- Sharp: I had a couple of questions about some letters that I saw. They dealt with some of your work, I presume, under Professor Woodford. The letters I told you about in the letter I sent you, one from the Palmer Corporation. They had sent some quartzite and other rock samples to Professor Woodford for analysis.
- Revelle: Yes, that's right.
- Sharp: I wonder why they had sent them to him especially.
- Revelle: Well, because they wanted him, or one of his students, to look at these thin sections and identify and describe them. I was supposed to do that, and I never really did it. It was after I came down here to Scripps. Woody was quite unhappy about that. I was too-- I'm still unhappy about it but I never did it.
- Sharp: Was that fairly common that different industrial companies would ask college professors for that kind of analysis?
- Revelle: Well, it wasn't done in any such formal way. Woody was a friend of some geologist who worked for the company. Woody had taken his Ph.D. in geology at Berkeley at a fairly late age. He graduated from Pomona in 1912 and took his Ph.D. about 1922 or so, something like that. So he was quite a mature graduate student. He wrote a famous thesis on a classic of California geology. [telephone interruption]##
- We were talking about the rocks, were we?
- Sharp: The rocks, and whether or not sending out samples like that for analysis to a--.
- Revelle: No, this was a purely personal thing on Woody's part, because one of his fellow graduate students at Berkeley had sent them to him. Woody was a specialist on what are called the Franciscan rocks out here, rocks which have several peculiar minerals in them. These rocks sent to him were somewhat like the rocks of the Franciscan formation; they were peridotites, very basic rocks full of magnesia and iron, not so much silica. I just never finished that job, one of the many black marks in my career.
- Sharp: I didn't mean to bring up a black mark.
- Revelle: That's okay.
- Sharp: Had you thought about really what careers were sort of open to you in terms of being a geology, at that point, a geology student?

Revelle: No, I've never been very good at thinking about the future.
What I thought I would do was to become a professor.

Sharp: An academic career.

Revelle: Yes, that's right.

Sharp: Not going into industry?

Revelle: No, I didn't have any intention of doing that.

II NOTES ON ELLEN CLARK REVELLE, GRADUATE SCHOOL AND EARLY RESEARCH EXPERIENCES

Revelle: I remember asking my wife, Ellen, when I proposed to her, would she be willing to be the wife of a professor. Which I take now as being pretty arrogant of me, but I guess it was easier to become a professor in those days than it is now.

Sharp: Yes, very much so.

I had some family questions for you. I thought we might talk a bit about your marriage, but before this, a few more questions about your parents. Now this was the first part of the Depression, of course.

Revelle: No, it wasn't. I graduated from college in '29. That was the summer in June of '29 when everything was rosy.

Sharp: Then following in October, that was pretty much it.

Revelle: That's right.

Sharp: I wondered if you remember those first couple of years after '29 and what effect the Depression had on your parents.

Revelle: Well, it didn't have any effect that I could see on my parents because my father was a teacher. They were not affected by the Depression, teachers weren't.

It had quite a bit of an effect on my wife's family. Would you like to talk about that a bit?

Sharp: Yes.

Revelle: She is the granddaughter of James E. Scripps, the founder of the Detroit News. Her grandfather had four children, three daughters and a son. Ellen's mother was Grace Scripps. She married a very difficult man named Rex Clark, who was in business in Detroit, went bankrupt and had a nervous breakdown. So they moved out here to California. They settled first on a ranch near Julian.

Ellen's eldest brother had already been born in Detroit. His name is Rex Scripps Clark. He's still alive, at the age of about eighty; he's an invalid and has been an invalid for thirty years or so. He has something like multiple sclerosis. Ellen Browning Scripps had moved here to La Jolla in 1895 or thereabouts. Ellen was born in 1910. She was born in Ellen Browning Scripps's guest house. This was an accident really, because her mother was visiting here in La Jolla and all of a sudden the baby started coming and they went into the guest house and, I think, she was pretty much delivered by her father with Aunt Ellen's help.

The family owned this newspaper in Detroit, the Detroit News. The publisher, the head man, was Gracie's brother-in-law, George Booth, who also had built a chain of his own in Michigan called the Booth Newspapers. This was a very prosperous newspaper in those days. The company was called the Evening News Association. Our principal income still comes from the Evening News Association.

Ellen's father had a tremendous drive to make good, but he had no business sense. One of the things he did was to settle in a town called Norco, near Corona. He was one of the founders, maybe the founder of Norco, which is now a fairly prosperous little rural community. They found hot springs there, and, in those days particularly, hot springs were believed to have some thermal, therapeutic value. So he built a big resort hotel called the Norconian Club, with a lake--an enormous place. It cost millions of dollars. He had very peculiar ideas about who should come to it. He didn't want any Jews, didn't want any drinking. This was, of course, during Prohibition. The end result was that he didn't have many customers. [laughs]

Sharp: Right.

Revelle: So he went, essentially went, broke. He had used Ellen's mother's Detroit News stock as collateral for his loans to build this club. So, in the early part of the Depression, Ellen's mother was living on \$200 a month, something like that.

Revelle: They got divorced about 1930, as I remember, 1929 or '30. He had fallen for his secretary, a woman named Jimmy. A very sad event in my mother-in-law's life. She married the captain of her yacht--they had a yacht called the Norconian.

He was a Swedish sailor named Gotfried Johanson. He was extremely loyal to her and wouldn't allow anyone to say a word against her of any kind. A very virtuous man but not very much education. He'd been in the Coast Guard and was a merchant ship captain. He was a good sailor. They got married more or less over the strong objections of the other members of the family. I remember we went back to Detroit and argued about it, in 1931.

Sharp: That was before you and Ellen were married?

Revelle: No, after we were married. We were married in June 1931. She was the niece, as I said, the niece of Ellen Browning Scripps as well as the granddaughter of James Scripps. So she was in the first class at Scripps College. She was a freshman when I was a junior. We never met in Pasadena; we met at a dance at Scripps College on Valentine's Day in 1928 during my junior year.

In college I did pretty well, socially. I was a member of one of the leading fraternities called Sigma Tau.

Sharp: Was she in a sorority, or did they have sororities?

Revelle: They didn't have any sororities at Scripps, and they only had local fraternities at Pomona. But, there was a group of us who were friends and I felt quite at home at Pomona and quite happy there. Several of us started going out with Scripps girls. As I say, our first meeting was at this dance.

She had a couple of other boyfriends from Pasadena. One summer she went back to Omaha and fell for a boy named Joe McClenaghan there, who was a hog farmer, or cattleman of some kind. He was really just nobody at all, but you know how girls are, and her mother was very, very tactful about this. She showed very good sense, and she asked this boy, Joe, to come out and visit them in Pasadena, which was the end of the romance. He was the cousin of one of Ellen's classmates at Scripps, Ruth McClenaghan.

Sharp: She'd gone there to visit Ruth?

Revelle: Yes, that's right.

Revelle: Ellen and I gradually got so we were more and more attached to each other. In the summer of 1929, they took the Norconian east on a trip to Maine and the East Coast. Ellen spent the summer back there with one of her friends, Caroline Comstack, who's now married and has been for forty years to one of my very good friends in Pomona, Rollin Eckis--one of Woody's students. Later he became president of the Richfield Oil Company.

When Ellen came back at the end of the summer she, I guess by that time, had decided she wanted to marry me. So she told me about her family's financial troubles on the theory that that would make me less embarrassed about her being a rich girl, saying that she didn't know whether she could go back to college or not because the family had gone broke. I remember what I said was "then you'd better marry a rich man." [laughs] Which was not at all what she and her mother had planned on.

But I thought better of it by the end of the summer--a few weeks later--and I then asked her whether she'd like to marry a professor, if she could live on a professor's salary, which she thought was a great idea.

Sharp: But then you waited until the following summer?

Revelle: Yes, well I stayed there for a year as a graduate student at Pomona, basically to be with Ellen. And Woody had gotten me a job as a teaching assistant for his geology--he was a one-man Geology Department.

Sharp: Yes, I think I had read that he was.

Revelle: But he always had one of his students as his teaching assistant, and it was traditional that each year he'd get one of his students a job as teaching assistant at Berkeley, so one after the other of his students got his Ph.D. at Berkeley.

Sharp: You mentioned to me last time we met, that originally when you got the assistantship at Berkeley, you felt embarrassed about it. You didn't feel that you deserved it. Then you decided that you did deserve it, and things were better after that.

Revelle: That's right. As I told you, I thought about it for years and realized finally that I really deserved that teaching assistantship because of what I learned from Woody, not much geology but I did learn the language. I learned how to read the literature, and I learned that most of the nature and history of the earth were not known, a feeling that nobody knew very much. But it was possible to find out a lot, through research, and it was

Revelle: just a lot of fun to do it. Those three things are the essence of what an undergraduate education, in geology, at least, ought to be about.

One of my problems all my life has been that I've never known enough mathematics or enough physics or chemistry, because in those days geologists didn't necessarily take basic sciences.

Sharp: A spread of sciences like that.

Revelle: The fundamental sciences. They did such things as mineralogy, petrology, structural geology, historical geology, and paleontology. All descriptive subjects but not really dealing with fundamental science. Quite different now--for example, we have no undergraduate geology at UCSD.

Sharp: Focusing instead on math and chemistry and physics.

Revelle: Yes, that's right. Physics.

Sharp: Once you got to Berkeley, what were the differences? What did you notice most about how the course of study was different from what you had gotten?

Revelle: Well, the big difference was that there were ten members of the Geology Department.

Sharp: The faculty members.

Revelle: The faculty members, yes, whereas there was one man at Pomona. He couldn't do much. [laughs]

Sharp: Was that a little terrifying at the beginning that there was all this--

Revelle: Well, it wasn't terrifying. But I learned a lot.

Sharp: And quickly, I imagine.

Revelle: I was always quite bright, so I didn't have much trouble with the courses. I remember one of the men who influenced me the most at Berkeley was a man named George Louderback, who was chairman of the department and a very thoughtful--skeptical teacher. He made you wonder whether anything you read was true or not, you had to really see what the logic was and what the evidence was. A wonderful training--I took a seminar from him; I've forgotten what the subject was, but the method, the teaching method, was so Socratic that it was really delightful.

Sharp: And he liked students?

Revelle: Yes.

He was one of the organizers of the academic senate in Berkeley. In the 1920s, you may have read that after Benjamin Ide Wheeler, who was an autocrat and a very dominant figure in Berkeley, the next president was a man named David Prescott Barrows, who was a Pomona College graduate, a political scientist. He just wasn't up to the job in the same way that Wheeler had been, and the faculty revolted.

Sharp: There was a real void there that they could--.

Revelle: Yes, that they could take advantage of Barrows' weakness, and they essentially made Berkeley the most faculty-run university in the country.

The academic senate has tremendous power, and Louderback was one of the organizers of this revolt and one of the organizers of the senate. There were several other famous professors at about that time who also took part in it, but the only one I remember was Louderback.

I was at Berkeley for just one year.

Sharp: Did you do some instructing there?

Revelle: Oh, sure, I was a teaching assistant.

Sharp: That's what I thought.

Revelle: I was one of the teaching assistants in an elementary geology course taught by a man named Norman Ethan Allen Hinds. Undergraduates thought he was the best teacher in Berkeley and I thought he was the worst--he was so unlike Woody. Woody, as I said, had taught us that not much was known about geology, but you could find out.

Sharp: That's a hard admission for a teacher.

Revelle: Whereas Hinds--to Hinds everything was known about geology, and it was all cut and dried and beautifully outlined.

Sharp: And he knew it?

Revelle: At least that's the way he taught it. So the students never got the idea that there was any fun about it, it was just a collection of facts to learn. For example, he had a diagram of

Revelle: the Grand Canyon, on which there are a whole series of strata and every one of those strata was identified and described and was all laid out right there beautifully before you. That's why the students liked it so much--it was so easy to follow. The fact was that damn near nothing was known about the Grand Canyon [laughs] --why it had gone up and down, why the rocks were there, even what the rocks were, what their age was, what their origin was. Just a tremendous ignorance really, but not the way he taught it.

But, anyhow, he and I and a man named Dana Russell, who was also a Pomona College graduate--one of Woody's earlier graduates--we became very good friends and used to cook dinner together a good deal of the time. I lived in the International House, but I didn't like to eat there and I avoided, I spent as little time in the International House as I possibly could [laughs] --which is not at all kosher because you were supposed to eat so many meals there. So, I never got along very well with the authorities in the International House [laughs] and I used to get up early in the morning and avoid them.

Sharp: Get out.

Revelle: Get out and come back late at night after they'd all gone to bed. There was also a special reason my sister and I had gone to Berkeley at the same time. She as a freshman and I as a graduate student. We drove up together. She had an old Buick and I had an even older and more decrepit Ford, which I had won in a lottery at Pomona for \$20 or something like that.

Sharp: It was falling apart?

Revelle: Well, it never ran after we got it to Berkeley. [laughs] I parked it in an International House parking lot and just deserted it there. And they didn't like that either.

Sharp: No, I wouldn't think so.

Revelle: I didn't have enough money to move it, so I just had to leave it there.

Sharp: Did you use your sister's car some?

Revelle: No, I didn't use it very much, because the International House is quite close, as you know, to the rest of the campus, so we walked. I remember I used to have a nine o'clock or 8:30 in Life Sciences Building, which I was teaching, and I used to just barely make it. I'd run all the way down there, and come in, "Haha Haha--" [panting] sweating and panting.

Sharp: At least it was downhill.

Revelle: At least it was downhill, that's right. [laughs] But Berkeley is a wonderful place, and I loved it. It was one of the great experiences of life to go to Berkeley--I still get a chill going up and down my spine whenever I go there.

Sharp: It's a very alive campus intellectually.

Revelle: Oh yes, and it was then too. Let me get some cigarettes.##
Ellen was still an undergraduate at Scripps College. She was in her senior year there, and this was a rather bad year for her, particularly, because I'm a very poor letter writer. I've always been a great procrastinator, and she didn't hear from me for months on end and she was very lonely and discouraged about this. She and her mother did come up to Berkeley during one of the vacation periods and stayed in the Durant Hotel. We had a good time then. I did borrow my sister's car at that time, and I remember one of the traumatic experiences we had was that we drove up to Strawberry Canyon. I was so innocent that I didn't know that that was a no-no to go up and neck in Strawberry Canyon. The campus police came by and flashed a light in our car and Ellen's hair was down, nothing more serious than that, but they really bawled us out something awful. We were quite shaken, I remember, when we went back to the--I tremblingly lit a cigarette I remember. It was a shocking experience.

The problem was, where could you sit and talk or do anything else in Berkeley? It was difficult.

Sharp: But she finished out the year then, and you were married after she graduated.

Revelle: We were married just a few days after she graduated.

Sharp: Did she have ideas that she wanted to work in any particular career, or did she want to be at home, or what did she want to do?

Revelle: Well, I don't think she ever thought much about having a career. She thought being married was career enough. She did major in psychology at Scripps College and had a teacher there whom she liked a lot named Mary Eyre [spells out], Mary B. Eyre. Also, another teacher named Una Sait, who was the wife of Edward Sait, the great political scientist who had left Berkeley under some kind of a cloud, because of some kind of a scandal. He taught at Pomona after that.

Ellen majored in child psychology and might very well have gone into some kind of psychological counseling if she hadn't gotten married.##

Revelle: In those days girls didn't often have a job if they were married.

Sharp: We have talked a bit about your coming down to La Jolla and the circumstances of being asked to analyze the Carnegie muds.

Revelle: We first went to Berkeley where I was taking both inter-session and summer session, taking the whole first two-year physics course. I'd taken the inter-session course in electricity and magnetism and sound and light. Of course, those were the two second year courses--I don't know why they gave them first, but they did. I was lost in that pretty much.

But during summer session we had mechanics and heat, the beginning physics, and of course, by that time, I had already learned it the hard way.

Then after summer session was over--we got married between inter-session and summer session--we drove to British Columbia in Ellen's Chrysler that she'd gotten as a wedding present from her Aunt Ellen Browning Scripps. We had a nice little honeymoon in a cabin that my Uncle Will owned. My Uncle Will, my mother's brother, had settled in Victoria and married an Englishwoman. She was my Aunt Lizzie; she was a Cockney and spoke Cockney English. [laughs] I remember when I introduced Ellen to her she said, "Is it Ellen with an H or Helen with an E?"

Then we drove down here. We moved into part of a house, Cottage 24, which is one of the bungalows at Scripps. We were rich because I had an income of \$100 a month as a research assistant, and Ellen got an income of \$25 a month from her Aunt Ellen's estate. So we lived much higher on the hog than any of the other graduate students, because of this extra \$25 a month.

By 1933, the family's fortunes had improved somewhat--quite a bit in fact, after Gracie's divorce from her husband, and after her marriage to Captain Johanson. So we got a share of the income from her grandfather's estate, I think \$200 or \$300 a month. In 1933 that was a lot of money.## [pause in tape]

Sharp: I thought we might talk a little bit about your research assistant work at Scripps besides the Carnegie muds--we talked about that some, and that it was your starting point with your work at Scripps. But, I wondered, if you wanted to talk about some of the other early projects that you might have worked on.

Then I thought we would talk about the trip to Bergen, Norway, and your study, the conference at Edinburgh, and a few other isolated topics.

Revelle: Well, one of my both strengths and weaknesses is always been that I get interested in a lot of different things, never perhaps pushing any one thing far enough.

When I began here as a graduate student, I guess I told you, there was another assistant to Dr. [T. Wayland] Vaughan, a man named Eldon Thorp. He was very good at taking micro photographs and thin sections for Dr. Vaughan's corals. I was very bad at both of those things, so, after a while, Dr. Vaughan didn't ask me to do anything for him anymore. [laughs] He relied on Thorpy to do everything, which was very good as far as I was concerned. It's what my friend, Giff [Gifford C.] Ewing, calls "dynamic incompetence."

One of the things Dr. Vaughan did ask me to do was to look at some bottom samples from the Bahamas, which had calcium carbonate precipitate on the bottom--two kinds of precipitate, one consisted of little spheres called oolites [spells out] and the other were needles, very small needles. I identified those needles as aragonite which is a form of calcium carbonate that is relatively soluble compared to calcite. I published a little paper on that--the first paper I ever published--on these aragonite needles from the Bahamas.

I thought they were probably precipitated inorganically, not by organisms. Most calcium carbonate is precipitated by some kind of shell-forming organism. I'm not sure how well that conclusion has held up, but it certainly looked as if they were deposited from solution by some ordinary chemical process.

That led me into a study of what's called the buffer mechanism of sea water.

There were three people involved with that at the time; one was David Greenberg of Berkeley, the other was Erik Moberg here at La Jolla, the third was a girl named Esther Allen. They asked me to look over the paper they'd written--there were some numbers they had that were hard to explain. I did look at it, and I looked at the possibility that boric acid was what was causing these discrepancies. That turned out to be true--the buffer mechanism is not only carbon dioxide and its ions, namely carbonate and bicarbonate, but also boric acid and its ions, two kinds of borate ions. So we published a paper together, Greenberg, Moberg, Revelle, and Allen, called the buffer mechanism of sea water.

Sharp: Was that the Vancouver paper?

Revelle: No, that was a paper published as a bulletin of the Scripps Institution of Oceanography. You can find it in the library; it's one of the early bulletins.

Revelle: We found that there were four other people working on exactly the same problem and who came to the same conclusions. Their names were Wattenberg, Gripenberg, Buch, and Harvey, so four out the eight people were berg.

Sharp: Sounds like a law firm.

Revelle: That's right. [laughs] They, I think, got most of the credit although the two investigations were conducted completely independently without knowledge of the other group doing it.

Sharp: Where were they?

Revelle: Well, Buch was in Finland, Wattenberg in Germany, Buch and Gripenberg--Gripenberg was a woman, Stenc Gripenberg--they were both in Finland. Harvey was at the Marine Biological Association in Plymouth, England. Wattenberg was with the Institute for Meereskunde in Berlin. He'd been the chemist on the Meteor Expedition during the 1920s, this was the first really comprehensive study of an ocean--in that case the South Atlantic Ocean.

That paper on the buffer mechanism of sea water was really, I thought at the time, an outstanding scientific accomplishment. I worked with Erik Moberg in revising the manuscript. He was a perfectionist. We spent literally months working on this, working over and over every word of this paper.

Sharp: Is that the kind of thing that you enjoy?

Revelle: Working over papers? Oh, yes, sure, it was fine. It was unfortunate that we spent so long at it because we would have published first, if he'd been willing to let it go a little bit earlier. [laughs]

Then Dick Fleming and I followed through on that calcium carbonate subject by studying the solubility of calcium carbonate in sea water. What we did was to have a big bottle, in which we put some sea water, and I think we seeded it with crystals of calcite, mostly ground up calcite although I'm not quite sure about that. We measured how much was precipitated, also how much was dissolved; we did it both ways. What we found was that the solubility product of calcium carbonate was such that the surface waters were greatly oversaturated. This has been a puzzle ever since, why this is. The present idea, I think, is the calcium forms complexes with organic matter in the water. But I remember one result of those experiments was that we found the needles just exactly like the ones in the Bahamas. That's one of the reasons

Revelle: I thought they were precipitated inorganically.

Then, in 1934, I made a cruise to the Gulf of Alaska and down to Hawaii on a navy ship, the U.S.S. Bushnell, which was the flag ship of the submarine force. There was just one U.S. navy submarine force at that time. We had a big submarine tender called Holland, and about six submarines, and this Bushnell, the flag ship.

The admiral in command of the submarine force was named Cyrus W. Cole; a nice little man, very mild-mannered as admirals often are. One thing I remember was that he and I both tended to get seasick when the ship pitched. The place where it pitched the least was in the fan tail. So we would both end up in the fan tail whenever we got into the open sea, [laughs] until we got accustomed to it.

We visited several Alaskan ports; including Sitka, Valdez, and Juneau--I remember those three particularly. Then we went out to the Aleutian Islands to Dutch Harbor, and from there we made a trip across the Pacific from Dutch Harbor to Hawaii. On this leg of the voyage, I made a series of what are called hydrographic stations, where we measured the temperature at various depths, collected water samples for salinity, and measured oxygen and phosphate and nitrite.

Sharp: This particular cruise on Bushnell, was that entirely sponsored by the navy, or was there some Scripps funding also?

Revelle: Well, all the navy did was to give me a stateroom, give me accommodations on board. We paid for all the--but, there wasn't much that cost very much. We had our water bottles and our chemical apparatus, and they gave me a place on board to set up the labs, and they gave me a pharmacist's mate as an assistant. I taught him how to measure phosphate and oxygen and he'd do that at every station.

I think we made about twenty stations from Dutch Harbor to Hawaii.

Sharp: I'm interested in, as I mentioned to you, your comments on the history of the relationship between the navy and scientists.

Revelle: Well, the Navy Hydrographic Office was always interested in helping us to get aboard one of their ships. For example, Dick Fleming was on the hydrographic survey ship Hannibal in the Gulf of Panama, and Bushnell made another trip later. But they never put any money into it; they just gave us space--not only space to sleep but also laboratory space, and often, as I said,

Revelle: assigned a pharmacist's mate or somebody like that to help with it, both in Dick's case and in mine.

Sharp: We can talk more about this once we get into your years in the navy, which we'll do tomorrow.

Revelle: But I made some very good friends on that cruise, one of them being the captain, a man named Abel T. Bidwell, another being Leo Bachman, who was the flag lieutenant, and a man named Pug Kirpatrick (all Kirpatricks in the navy are apparently called Pug for some reason). And several other officers whose names I now no longer remember.

The result of that was we ended up in Hawaii, and we spent about a month there. Ellen came out to Hawaii on the Matson Liner Lurline. Then after a week or so she and I sailed back to the mainland. The Bushnell remained in Pearl Harbor; consequently we saw a lot of our new navy friends also. They urged me to apply for a commission as a reserve officer in the navy. One of the problems was that, then as now, I tend to have high blood pressure, and I wouldn't have passed the physical exam except that Captain Bidwell kind of pushed to let the medical officers let me get by. I was also a little bit too tall; you're not supposed to be over six feet four, but they shrunk me a little. [laughs]

So I got this commission as a reserve officer in 1936--never having had any military training at all, either before or after. All during the war, I still saluted with the Boy Scout salute.

III EUROPEAN TRAVEL, 1936: GREAT BRITAIN AND NORWAY

Revelle: Then we prepared a whole series of papers, Moberg, Fleming, and I, for the Edinburgh meeting of the International Union of Geodesy and Geophysics, the International Association of Physical Oceanography. I've forgotten what the subjects were, but I remember we talked about the distribution of oxygen, and phosphate in the water masses of the North Pacific. I think I gave half a dozen papers at that Edinburgh meeting.

Sharp: That was surprising to me that you had done so many all at one meeting.

Revelle: Yes.

Sharp: They were all somewhat related.

Revelle: Yes, they were. They were all about the oceanography of the North Pacific.

Sharp: That, in fact, was a large delegation from Scripps--I counted twelve--.

Revelle: No, there wasn't. I don't think there was anybody but me. At that meeting in Edinburgh; that was in 1936, September '36. That was the first trip Ellen and I ever took outside the United States. We sailed to London--Southampton, actually, then went to London and spent a day or so there.

Revelle: One man I remember whom we met on the ship was a British Army officer, named Captain Westray Battle Hancock. We corresponded with him for years; he was even taller than I was. He was in the British army--something happened to him during the war; I never knew what or how he ended up.

During this IUGG meeting in Edinburgh, I became good friends with Colonel R.B. Seymour Sewell and John D.H. Wiseman of the British Museum, who had just finished an expedition to the Indian Ocean. I think it was on a boat called the Mahabis. They'd written that up, or were in the process of writing it up. This was done in the early 1930s, this Indian Ocean expedition. They got some long cores, and I believe this fellow Wiseman spent the rest of his life looking at one core. He was in the British Museum.

I visited the Sewells in Cambridge, and that was really my first experience with English hospitality; they are very hospitable if they like you--if they know you; they're usually very shy. He had two daughters, I remember, very soft-spoken and diffident.

Then we went to Bergen.

Sharp: Did you come back to the United States before you went to Bergen?

Revelle: No, no, we went directly to Bergen. Ellen went right after the meeting, and I went first to Cambridge to visit Colonel Sewell. So then I caught the boat from Newcastle about a week after she did.

Sharp: If I could just stop you, I had a couple of questions about the trip to Edinburgh--more about the experience of being at an international, and important, meeting as that was and how you would describe the exchange of information among the different scientists there--just what it was like at that period.

Revelle: Well, it was very nice because the International Association of Physical Oceanography was so small--there were only about thirty people at that meeting. [laughs] You understand what the IUGG is? It's the International Union of Geodesy and Geophysics, and it has a series of associations that make it up, such as the International Associations of Meteorology, of Oceanography, of Terrestrial Magnetism. I think they had one on volcanology and the biggest one was the Association of Geodesy. They are the people who precisely survey the earth, the figure of the earth, the acceleration of gravity and things like that.

Among the people I remember at the meeting were Dr. Bjorn Helland-Hansen of Norway; (I think he was the president of the association, and I later worked with him in Norway.) Joseph Proudman, who was from Liverpool, head of the Tidal Institute in

Revelle: Liverpool, a great theoretical oceanographer. Helland-Hansen brought his ship, Armaner Hansen. He was the director of the Geophysical Institute in Bergen. He brought his ship, Armaner Hansen to Edinburgh. We took a day's cruise on it. I remember Joseph Proudman was very seasick but before he got seasick he told us some wonderful stories about Isaac Newton.

Sharp: Because it was so small did that make it necessarily more informal?

Revelle: Well, actually, the program consisted of the presentation of papers, just like it does now, and the discussion of the results. No real difference except for size between what's done now and what was done then. The abstracts, brief papers, were published in one volume of "Rapports et Procès Verbaux" as they called it.

Sharp: It was published afterwards or before?

Revelle: Afterwards. The Moberg, Fleming, and Revelle papers were all in that Report.

Sharp: The National Academy of Science and the National Research Council I think had some relationship with the IUGG, didn't it? At least the names of NAS and NRC were on some of the programs that I saw, but I wasn't really sure what the relationship was.

Revelle: Nor am I. The adhering bodies to the International Council of Scientific Unions are academies. Each international union doesn't have any personal human members per se, it has academies as members. So our academy now has committees for each one of these unions. I don't think there was any special relationship.

Sharp: I just wondered about the role of the National Academy of Science in this early period, and what it was doing.

Revelle: Well, it didn't do very much--not until Det Bronk became president.

By that time I had become an instructor, and gotten my salary raised from \$100 a month to \$150 a month. That was in the fall of 1936--I got my Ph.D. in the spring of 1936 at Berkeley. Since it was quite obvious that I was not going to leave [laughs] they made me an instructor until the war came along.

Sharp: Scripps was not granting degrees at this point?

Revelle: We were part of the Graduate Council at Berkeley, that is the graduate division at Berkeley. The only part of the university that officially granted doctor's degrees was Berkeley. So I went up to commencement at Berkeley and was given my degree by Robert Gordon Sproul in the Berkeley Stadium.

Revelle: the doctoral committee was a Berkeley committee, with a couple of La Jolla guys on it--also somebody from Riverside, William C. Kelley, (Riverside, at that time was the Citrus Experiment Station--that was the only thing at Riverside), and Uncle George Lauderback, the one I told you about earlier. They were a very sympathetic committee; I had no problem with them.

The thesis was on these Carnegie muds.

Sharp: Right, so you got some good use out of them.

Revelle: Well, that was what I was supposed to be doing all the time instead of all these other things.

Sharp: Maybe we'll just go ahead and talk about the trip to Bergen.##

In the other oral history interviews that I told you that I found, the Texas A & M set, you mentioned that there was a very important group in Bergen whom you met, and whom you worked with. I'm not going to pronounce these names right probably, but--.

Revelle: Sverre Petterson, later became a Professor at Chicago and MIT. Jack Bjerknes--later became a professor at UCLA--one of the world's great meteorologists.

Sharp: And Petterson was?

Revelle: At that time he was head of the Norwegian Weather Bureau. Haakon Mosby. [spells out] Helland-Hansen himself, of course, and a man named Olaf Davik whom I didn't really work with, but he was a member of the staff there. My particular friend was Odd Dahl [spells out]. He was not a scientist; he was an engineer, and he'd been on the Maud Expedition. They'd taken him along as an aviator. The Maud Expedition was Amundsen's expedition to the arctic, which lasted for seven years. Harold Sverdrup was--

Sharp: He was on that one.

Revelle: He was on the whole darn time.

Sharp: The ship was actually frozen into the ice.

Revelle: Yes, they were trying to drift across the polar sea, but they never really got very far from the Siberian coast. They made all kinds of geophysical observations--of the aurora and of terrestrial magnetism, atmospheric electricity, oceanography, meteorology. All really done by Sverdrup; he was the principal scientist on board. He was the first genuine physical scientist who ever came to the Scripps Institution, a modern geophysicist.

Sharp: How would you describe the impact of all the work that was done at the Geophysical Institute on your own work, on your own progress in oceanography?

Revelle: Well, I would have done better to stay at Scripps because Sverdrup came to Scripps the summer that I went to Edinburgh and then to Bergen. I would have learned more from him. Helland-Hansen was a very busy man; he never had much time. I only saw him about once a month.

I worked on the current convergence and divergence in the surface currents. I never published a paper on it, though.

But what I learned was mostly about people and life; it was a very valuable and important experience. In fact, Ellen was so taken by Norway that she wept when we left.

Sharp: I just found this yesterday--this little paper that I'm pretty sure you wrote.*

Revelle: [studying paper] I must have written this because it says "both my wife and I" and it's also my style pretty much. I write somewhat better than she does.

Sharp: There are some wonderful observations about what you thought about being there and what--

Revelle: Yes.

Sharp: I wondered if you remember at all how you came to write it?

Revelle: I don't remember. I haven't any idea how I wrote it, or when I wrote it. It was obviously just about the time we were there.

*"Paper on Experiences in Norway by Roger Revelle," Revelle Papers, SIO, 81-23, Box 3. See following pages for this paper.

We lived in a house in the country near Bergen, in a suburb called Paradis. This is right next to another suburb called Hop. The opportunities for puns in this situation were obvious, of course, and became really enormous to a person of talent. Our landlady was the mother of two sons, one an author, the other a publisher, and her husband was the first cousin of Edvard Grieg, the composer, so that we lived in the midst of considerable culture.

The corollary to this situation was however that we also lived in the midst of some of the most atrocious plumbing it has even been my misfortune to see. None of the wash basins held water, the hot water system emitted groans of agony every time it grudgingly yielded a few scanty drops of tepid, greenish colored liquid, the water closets can hardly be described in printable language. When they flushed at all it was only after prolonged wheedling, and then the darned things wouldn't unflush, but went on wheezing and gurgling far into the night.

The simple fact is that the Norwegians, like the English, the Scotch, the Swedes and probably other European races are not gifted plumbers. One of the major American contributions to Western civilization has been our development of the art of plumbing. And don't let anybody tell you that good plumbing hasn't got a good deal of spiritual significance.

There was an interesting variety of homes and people in this part of Bergen. It had originally been farm land but had mostly been subdivided and built up into the fine homes for well to do city people. There were still quite a few of the little farms left, right in the midst of the big houses with their lovely gardens. We bought our milk from one such farm, down the road about fifty yards from our house. The sheep from there were a neighborhood menace, as they wandered around freely most of the time hoping they could find the gate to someone's flower garden open so that they could vary their grass diet with a few bulbs or berry bushes. We would all run to the window at the sound of tinkling bells to see if the pests were in our garden. If they were we would dash out to try to hurry them away, but they were so sunk in iniquity that they would continue calmly to steal bites on their leisurely way out, always maintaining that bland, innocent sheep expression.

In spite of these minor domestic inconveniences, we fell in love with both Norway and the Norwegians. Bergen is a delightful city, very old and very clean, and full of fine big blue eyed people. Among them is a high percentage of pretty girls, especially if you are one of those gentlemen who prefers blondes. The city is set in a bowl-like hollow between the mountains and the waters of a many-branched fjord, in such a way that both the forests and the sea seem to come right down into the city itself and to be an intimate part of its life. The waterfront, unlike that of many English and American cities, in which the docks are reached through miles of badly paved streets and ugly buildings, is right in the center of town, surrounded by hotels and banks and shops. It is quite a sight to come out of a department store after buying something so prosaic as a suit of woolen underwear and almost step on deck of a little coastwise steamer, or see the big hull of a TransAtlantic ship

chuffing slowly to its berth. On the other side of the city, the forests and mountains are so close that many of the houses have waterfalls in their own back yards.

The Norwegians all admit that Bergen has a miserable climate, because of the great amount of rain (It rains about seventy inches a year, but this doesn't seem so bad to a Californian, brought up to regard fresh water in any form as a blessing.). In actual fact the climate is very mild and it is usually necessary to go nearly as far from Bergen to find snow for skiing as it is in San Diego. Occasionally, however, it does snow, sufficiently for skiing right in the streets, and then the whole town is practically turned upside down by sheer joy. This happened once last winter while we were there. The schools were closed for several days, the Institute where I was working declared a day's holiday, and many of the shops and companies which didn't close entirely sent half their employees to the best ski spots one day and the other half the next. Street cars going to hilly parts of the town fairly bristled with rows of skis on both sides, so that they looked from a distance like half-bald porcupines.

The most interesting people we saw in Norway were the bonders or peasants, and the fishermen and their womenfolk, who, especially when they are middle-aged or older often have very strong and deeply lined characteristic faces and gnarled bodies.

The fishermen are usually great strong fellows dressed in faded blue to match their eyes, often with round, child-like faces with wide-apart eyes and prominent cheek bones. It is easy to imagine them in the place of their ancestors, solemnly rowing a dragon ship on a Viking expedition under the leadership of such a man as Nansen or Amundsen, the great Norwegian explorers.

Their lives are very hard, as most of the land in the country cannot be cultivated, being chiefly barerock. Every available bit of soil, whether high up on a cliff or on a lonely or otherwise desolate island, or in a more pleasant location, is cultivated to the limit and has been for thousands of years. It is very strange to go through such a wild country, very much like parts of Alaska, and find settlements everywhere, in which the population and essential way of life has hardly changed since the Viking age, a thousand years ago. This is not to say that they still wear the same kind of clothes, or have open fires in the center of the room, with the smoke coming up through a hole in the roof, for there is electricity and electric light almost everywhere, and they have stoves and wear common garden-variety clothes, much like American farmers and fishermen; but the roofs of many of the houses are covered with sod and grass, and flowers in the spring. The peasants even carry daggers in their belts, and many of the country parties and dances still end in fights. It was only a short time ago that the quaint custom ended of a wife carrying her husband's shroud to a party, just to be prepared.

We felt very much at home with the Norwegians; they are frank, straightforward, simple and friendly, rather more formal in their social

relations than Americans, but with practically none of our pseudo-chivalry between the sexes, for example they do not rise when a woman comes in the room; however they always come up to their host of the afternoon before when they see him the next day and say "Tak for sist," meaning "thanks for the last time."

Both my wife and I felt more at ease with the Norwegians than with any other European people, even the English who rather awe us Americans. They are hospitable and kind, simple in taste and way of living, both inwardly and outwardly, lacking in the spontaneous reckless humor of many Americans; but the scientists and their wives who formed our circle of friends were much more civilized and well rounded than most American scientists.

There are no slums and there is little real poverty in the towns of Western Norway. There is none of the degradation and reckless hopelessness which one sees in England and our Eastern states. By the same token there is little of the brilliant luxury of Manhattan or of the West End of London; on the contrary no great extremes of wealth or poverty. There are many rich people, of course, particularly the numerous ship owners and the great merchants, but they are a careful hard headed tight-fisted lot, much like their ancestors, the Hanseatic merchants, or like the Yankee traders must have been.

Another time I wandered alone through the Bergen Hobise, the old fortress of Bergen, and the former palace of the kings in Viking days. One of the stone buildings there was built by Haakon Haakouson, one of the last Viking kings, in 1160. It fell on bad times and was used as a store-house after that, and was used as a store-house for trading goods by the German merchants who infested Norway from the fifteenth to the 18th century and occasionally as a battery for artillery, notably in a battle between the English and the Dutch in the early eighteenth century. People had completely forgotten its origin and history until about a hundred years ago when Norway regained her independence from Denmark and a great nationalistic revival of Norse culture took place. It has recently been restored, more or less according to the descriptions in the sagas, but in general following the awful taste of the Victorian era. A later restoration and redecoration, finished in 1916 under the direction of an artist sensitive to the beauty and simplicity of the ancient Norsk art, has considerably improved it, but the job has been too well done: only occasionally does one get the feeling of romance and mystery, of contact with things long past, which comes from broken stone walls and worn steps and black wooden doors. In the farthest room in the cellar, however, there was a pile of old stones, which have been dug up in the neighborhood. In the half darkness from a little barred window you could see broken capitals and benches, gargoyles of human and animal heads, some showing forth very prominently in the shadowy light, others only vague remains of eyes and noses. There were polished floor tiles there also, and faintly carved flat stone surfaces from the inner walls, and slates from the roofs. Imagine, if you please, my feelings on picking up such a stone head, carrying it over to the light, and trying to think of the forgotten winter when it was carved.

Afterwards I walked back along the Gamle Tyskebrygge, the old German quay. Here there is a long row of old wooden buildings with sagging roofs covered by blackened tiles and with leaning, propped up sides, which are still used as warehouses and shops. Many of these were built about 1710, after one of the great fires in Bergen. They are separated from each other by narrow dark passageways several hundred feet long in which one can get only glimpses of the sky between overhanging eaves and gables. Over each entrance there is a carved wooden figure of some sort: a man's head with many faces, a primitive statue of a viking warrior painted in natural colors, an angel in flowing robes, the head of a deer or a leaping stag. These are apparently the signs of the original owners. The shops here, like those of most waterfronts contain a fascinating variety of things, and along the wooden floors of the passageways you can often see long lines of barrels being rolled and thumped along by little men, or stacked in neat rows. Many of the buildings, which are usually about three stories high, go in a flat plane from the roof right down into the water in a fashion which is quite common in Norway and which, together with their extreme tidiness, gives a very characteristic appearance to Norwegian waterfronts.

One day I walked down to the docks to mail a letter to America by way of England, on the famous North Sea ship Venus, going to Newcastle. The ship is supposed to leave at 11:30 but Norway is still democratic, so the trains and ships preserve a certain liberty and do not come and go with the heartless precision which is supposed to be an acceptable substitute for freedom in Mussolini and Hitler land. In short you can be practically sure of catching that ship at 20 minutes to 12. It is still a sporting proposition at a quarter of 12. The quai was full of all the rattle and shouts and business which precede a great liner's departure. Soon the last line was cast off and the ship glided quietly away, shortly to disappear around the gray shoulder of the fjord into the skerries or small rocky islands which guard the western coast of Norway. Then I prowled around the docks, peering at lables of barrels and boxes, trying to look nonchalant when eyed by suspicious customs guard, gaping at fat flat barges, little coastwise steamships and big ocean-going freighters, and snub-nosed fishing boats with stained unpainted wooden sides and wooden nails. There were many things at the docks, being loaded and unloaded by the big and little ships, and carried back and forth by trucks: barrels of salted herring from Tromsø and Hammerfest, fresh cod packed with ice in flat wooden boxes, bound for Hamburg and Antwerp, sacks of dried fish for Brazil, burlap wrapped bales of cotton from America, sheet steel from New York, barrel after barrell of the many products of the Standard Oil Company of New Jersey, shining tiers of galvanized tin buckets, casks of olive oil from Marseilles, onions from Holland, lemons and shelled nuts, and canned tomatoes from Italy; grapes from Spain, packed with ground cork in kegs bound with split birch saplings, boxes of frozen whale meat, potatoes and sardines from Trondjem, and goats' milk cheese from Sognefjord; West Indian bananas, S. African oranges, pears and apples from Norway, and many other things with undecipherable names from all the earth's far corners.

We lived much closer geographically to the theatre of hate and fear which the newspapers in Norway, as well as in America, picture as a

modern European political scene, but we seemed as completely isolated from it as in California. As a matter of fact I suspect that most of the people living in other European countries have the same feeling of isolation, of the unreality of maps and general staffs, and those queer beasts called Nations. Most people really believe only in individuals of different races, and in the petty, mean details of living. It is only the statesmen and politicians who have the larger view, who can visualize with awful clearness the perils which encircle the fatherland, and which threaten the very foundations of western civilization, etc., etc., word without end.

Of course everyone makes generalizations about others peoples: the English have bad teeth, the Germans have no back to their heads, all Frenchmen look alike, the Dutch are fat and jolly, the Belgians are thin and don't bathe, the Americans are interested only in dollars; but these are used mostly for conversational purposes and nobody cares very much.

On the other hand there is a good deal of genuine idealism on both sides in the Spanish civil war, with the logical result that this struggle is characterized by more brutal passion and beastliness than has existed since the wars of the reformation.

Sharp: Because it's so detailed.

Revelle: I should point out to you that the description of the plumbing in Fran Grieg's house is largely imaginary, as Ellen said with some indignation when she read this just now. She thought the rest of the piece was pretty accurate.

I was fascinated by the number of things that came in by ship to Bergen: oranges from Spain, and grapefruit from somewhere else, and fruits and vegetables of many kinds. The only local vegetables they had in the winter were Brussels sprouts, and potatoes, and carrots that would keep over the winter in their cellars. All the fresh fruit and vegetables came from somewhere else.

[Reading paper] Here we are--"There were many things at the docks, being loaded and unloaded by the big and little ships . . . barrels of salted herring from Tromsø and Hammerfest, fresh cod packed with ice in flat wooden boxes . . . sacks of dried fish . . . burlap wrapped bales of cotton . . . sheet steel from New York, barrel after barrel of the many products of the Standard Oil Company of New Jersey, shining tiers of galvanized tin buckets, casks of olive oil from Marseilles, onions from Holland, lemons and shelled nuts and canned tomatoes from Italy, grapes from Spain. . . ." I remember making this list of things that I saw on the dock. "Boxes of frozen whale meat, potatoes and sardines from Trondjem, goats' milk cheese from Sognefjord, West Indian bananas, South African oranges, pears and apples from Norway and many other things with undecipherable names from all the earth's far corners."*

This is a really very nice little story, isn't it?

Sharp: It is. I was very happy to find it. [laughs] Have you found that you always seem to jot a few more personal observations, descriptions of life on these expeditions and the trips that you go on?

Revelle: I did it sometimes when I went on expeditions, yes. If you look at Helen Raitt's Exploring the Deep Pacific**--several parts of that were written by me, one way or the other, besides the introduction. For example, I sat down with her one day and dictated something about Polynesians, which is in that book.

*"Paper on Experiences in Norway," p. 32d.

**W.W. Norton, New York, 1956.

Sharp: That's on Capricorn.

Revelle: Yes, that was on Capricorn, right.

Sharp: Yes, I thought so.

Revelle: I amused myself on that trip by dictating to her various things, including this thing about the Polynesians. But it's a nice little story about Bergen, and some interesting observations. I see right at the end I said, "Of course, everyone makes generalizations about other people: the English have bad teeth, the Germans have no backs to their heads; all Frenchmen look alike, the Dutch are fat and jolly, the Belgians are thin and don't bathe, the Americans are interested only in dollars. . . ."* [laughs]

Sharp: Would you change that now?

Revelle: No, I think it's probably pretty reasonable, except that the British have better teeth now. They had very bad teeth in those days because they didn't get enough to eat in the 1930s, particularly the poorer people in England--they were very badly off.

But when you say something more personal than that--?

Sharp: Well, I wondered if you often got a chance to write personal observations on the expeditions. When I saw the Raitt book I wondered if you had some other sorts of journals that you began to write into.

Revelle: I never really kept any journals. I just wrote pieces like this.

There were somewhat more personal experiences in that we became very good friends with the Bjerkneses. Hedwig Bjerknes is still alive; she lives in Los Angeles now. They had two children, and we had two children, about the same age--Vil (Vilhelm) was a little bit older than Annie, and Kirsten was about the same age as Annie.

We spent a lot of time at their house, and they spent some time at our house, and we made many boat trips together. They had a little motor boat. We went on picnics in the day time, very often, and on other trips. There are some wonderful places around Bergen to do that--to camp out or to spend the night outdoors, more or less.

Sharp: They would guide you around?

*"Paper on Experiences in Norway," p. 32e.

Revelle: Yes.

Sharp: Was Anne in school?

Revelle: Well, she was only four years old.

Sharp: Oh, she was four. I was thinking she was a little older.

Revelle: And Mary was just about one year old.

One thing I do remember--Mary became very peaked and we were worried about her health. She just wasn't gaining weight. We were worried that she wouldn't survive, and I remember, one night particularly, she seemed to be in bad shape and I took the street car into town to find a doctor.

We found a woman doctor--her name was Dr. Skram-Anderson--she had a double name as Norwegians often do. She was very comforting; I was very worried, taking this night trip to Bergen and back again. She said to give her this fruit juice, they call it saft. I'm not quite sure what it is but it seems to be a combination of different fruit juices. They have some berries called lingonberries in Norway, much like our cranberries, and I think it was mostly lingonberry juice. She said, "She'll get better." And, by God, she did get better. [laughs] After that, we were not worried about her anymore.

Sharp: Did you keep giving her the juice or--

Revelle: Yes. She liked the juice.

She's a fine, healthy girl now but she was very puny then.

Sharp: I think that would be pretty scary.

Revelle: It was in a foreign country.

Sharp: Did you have enough facility in the language that you knew your way around?

Revelle: Well, so many people in Norway spoke English. The people at the Geophysical Institute all spoke English; people in the shops mostly didn't so we had to learn some Norwegian. We did learn enough to make our way around. Annie got so that she spoke English with a strong Norwegian accent.

Sharp: She probably was able to learn more words--.

Revelle: Well, she actually spoke Norwegian as if she were a Norwegian. When we got back here, after a year away, she went back to her school in La Jolla called the Balmer School, sort of a nursery and lower grade school, and some of her friends said, "Anne Revelle is back and she talks funny." Annie heard this and she was so embarrassed that she forgot every word of Norwegian within a week. [laughs] She never has remembered any of it since.

Sharp: It's difficult to be different when you're--

Revelle: When you're four years old.

Sharp: I wonder, the kinds of things that we've talked about today, if they prompt any conclusions on your part about what some of the most important events were of this 1929-1937 period that we've outlined here?

Revelle: I'm perhaps not introspective enough to have thought about that. One thing that was important was that I took to oceanography like a duck to water, and that's not such a bad figure of speech because it was water. [laughs] One reason for that being that you didn't have to climb cliffs as a geologist does--I've always been scared of heights. On the other hand, I made myself climb the mast on E.W. Scripps, and stand up on the upper top rigging of the mast even when the ship was swaying quite a bit, just because of this fear of heights.

But we had, before 1936, a little converted purse seiner called the Scripps and after a couple of years I became her captain. Whenever we went out on a "long trip" (actually one or two weeks) I would be the senior person on board. I took an examination first, for what was called a small boat license. A small boat was defined as anything under sixty-five feet long. Scripps was sixty-four feet, eleven and a half inches. [laughs] So it could fit in the small boat category. That was a very useful maturing experience, having that job, because no matter what the size of the boat is, there's a lot of responsibility.

Sharp; Just in maintaining it--.

Revelle: Well, coming in to anchor for example and not running into something. We had two professional sailors on board, Murdy Ross, the engineer, and a man named Frank who was the cook, and they, of course, helped a lot. But, they at least let me feel that I was in charge.

That in some way was a, I wouldn't say a transforming experience, but the thing that helped me to be a boss later in life.

Revelle: I guess the trip on Bushnell was a very important thing, too, being with a whole different kind of people, naval officers, getting some idea of what motivated them and what they thought was important.

The trip to Norway was certainly one of the great things of our young lives in terms of what you might call socializing. By that I mean what is meant when you talk about socializing children. That Norwegian year socialized us quite a bit.

Sharp: And getting in and finding your way around in a foreign country.

Revelle: Well, making friends with the people, the scientists there. Then we became very good friends, particularly with the Petterssons and the Bjerkneses, to a lesser extent with the Mosbys and the Fjelstads, and much less with Helland-Hansen because he was a remote, grand figure.

Sharp: He was older, he was of a--

Revelle: Previous generation. He was fifty-five or sixty years old then, I think. I was only, let's see, this was 1937, twenty-eight years old, and Ellen was twenty-seven.

Sharp: What about the scientific associations? Were they important?

Revelle: I'm not sure I understand what you mean.

Sharp: Well, now the role of scientific associations and scientific organizations, much like historical associations that I belong to, they're a way of getting contacts with people sometimes for new jobs, sometimes for exchange of information--especially for a young professional, helping you to start climbing the ladder. Is that how you thought about, for example, the IUGG? Is that how you thought about that association and some of the others that you began to--.

Revelle: Well, it was a chance to show what we had done, to put us on a world stage, which I guess we had never done before at Scripps. I didn't regard it as a way to get a job, but I did regard it as a way to become a--not exactly a world class scientist--but a world knowledgeable scientist.

Another man there was Columbus Iselin, I remember, at the meeting. He was pushing a pet project of his, the Bermuda Biological Station.

Sharp: Was he at the Woods Hole Oceanographic Institution at that point or is that later?

- Revelle: Yes, he was actually director at Woods Hole at that point. Or if he wasn't, maybe [Henry] Bigelow was director, but Columbus was assistant director and really ran what there was of it. It wasn't much of a place; neither was Scripps.
- Sharp: I know that when I give historical papers sometimes I'm a little fearful on my results and my conclusions--that I sort of put myself out there and the criticism that comes back sometimes is a little bruising. I wondered if you had ever felt that way, or you were totally sure of the conclusions, especially with this large number of papers at Edinburgh.
- Revelle: No, there were all quite descriptive papers. There wasn't any problem about accuracy. Looking back on them now you can ask a lot of questions about what they meant, but nobody cared very much about what they meant then, it was just what were the facts?, what were the data?
- Helland-Hansen had visited here at Scripps before we went to Edinburgh. If you look at Raitt and Moulton's First Fifty Years* at Scripps, you'll see a letter from him in which he discussed who should be the next director after T. Wayland Vaughan's retirement. Did you run across that?
- Sharp: Not yet, no.
- Revelle: He suggested at that time, though of course I didn't know it, that I'd be quite suitable for the job, but I was a little bit young.
- Sharp: What year was this?
- Revelle: That was early '36--same year we went to Edinburgh. So I'd run into him before. He was a wonderful man. One of the interesting things about him was he had no fingers. He'd gone on an expedition to northern Norway when he was a college graduate student, or just after he'd been a graduate student, and his fingers had all gotten frozen. He lost them. His fingers were only about so long (less than an inch). That was all that was left of them.
- Sharp: On both hands?
- Revelle: Yes. But he could light a cigarette or do anything with those little tiny stumps of fingers.

*Helen Raitt and Beatrice Moulton, Scripps Institution of Oceanography: First Fifty Years (n.p.: Ward Ritchie Press, 1967).

Sharp: Could he write?

Revelle: Yes, he could write and he could shake your hand so hard that it hurt afterwards. [laughs]

Sharp: He was really making up for it.

Revelle: Yes, that's right. He'd been a collaborator of Fridjof Nansen and also with a Swede named Walfried Ekman. Ekman is one of the great figures in oceanography. He invented something called the Ekman Spiral.

We spent the summer in Norfjord where Helland-Hansen had a summer house. Ekman was there most of that summer, too, so I got a lot of interesting contacts with him. One of my problems was, however, that I didn't know enough mathematics.

Sharp: You mentioned that before that you missed--a surprise to me that there were gaps.

Revelle: Big gaps.

Sharp: That you had to make up.

Revelle: Well, I never really made them up. There were big gaps in my theoretical understanding, and Ekman was a theoretical oceanographer, so I just barely understood what he was talking about.

Sharp: Were you able to ask questions that didn't betray too much of what you didn't know?

Revelle: That's right. [laughs] That's, of course, one of the great talents of the uneducated--you have to pretend that you aren't uneducated. But I've always been able to think physically fairly well, in other words to see through what's actually happening as opposed to seeing the mathematics of it. That's how I've gotten by, it's by trying to think of what the physical processes are.##

Sharp: So far the people that you've mentioned have been Europeans, Scandinavians really, for the most part, in terms of the leaders in their fields who made up oceanography or oceanographic study. There weren't too many Americans there.

Revelle: No, there weren't many Americans who really understood theoretical physical oceanography. As I remember, among the first ones here were [Harald U.] Sverdrup and [Carl-Gustaf] Rossby, both Scandinavians who spent part of their lives in America.

There were some Americans at Woods Hole, particularly a man named Ray Montgomery who understood it. But Sverdrup and Rossby were the ones who really brought hydrodynamics and geophysical theory into America in oceanography.

Some of our people were pretty good chemists--Dick [Richard H.] Fleming, Tommy [Thomas G.] Thompson Erik [G.] Moberg, Norris [W.] Rakestraw--those four particularly.

There was a bacteriologist named Waksman at Woods Hole who understood about microbiology.

Here at Scripps we had Claude [E.] Zobell and Denis [L.] Fox who understood chemistry pretty well.

We had some good marine geologists in the United States--Henry Stetson at Woods Hole, Francis [P.] Shepard here.

Some people got interested in it from other fields, like Milton Bramlette, and a man named [Wilmot H.] Bradley in the U.S. Geological Survey who looked at the Piggot cores from the Atlantic. They were collecting them in the 1930s. A man named Charles Piggot developed a coring gun, that shot a core barrel into the mud. They got fairly long cores with those.

Maurice Ewing was getting started at that time in Marine geophysics--seismology and gravity. Harry Hess at Princeton was also interested.

Sharp: So there were some major scientists around but not too much in exactly oceanography.

Revelle: Well, that's all oceanography but not theoretical physical oceanography, which is really a kind of hydrodynamics.

Sharp: That's what Sverdrup was--.

Revelle: That's what he specialized in.

We also had some good biologists. Here at Scripps we had Martin [W.] Johnson. Woods Hole had Henry Bigelow, Mary Sears--they were mostly related to taxonomy and ecology. Alfred Redfield, particularly--he was professor of physiology at Harvard and he left and went down to Woods Hole--was a great physiological biologist.

Revelle: So in every field but theoretical oceanography, or physical oceanography, the Americans did pretty well.

Sharp: The last question I had, I'm not really sure how to phrase it, but I'm wondering if you could just make some comments on how the terminology has changed.

I had seen a letter from 1940 that you had gotten from Siemon Muller. He had written it to you after a GSA [Geological Society of America] meeting in Los Angeles. He wrote the letter about a scientist named Schenck, his work on stratigraphic terminology. I guess you had a disagreement with Muller about the terminology, and what you both thought about it was somewhat different. But I wondered about that, you know, what sort of a problem it was and how you might have thought about it over the years.

Revelle: Well, one example I guess is the classification of deep sea sediments. In my doctoral thesis, I made up a classification which was nothing very original; it was based on the Challenger classification, dividing the sediments into terrigenous or terrestrial sediments--sediments that were mostly derived from the land and sediments that were in large part derived from the ocean, pelagic sediments. The pelagic sediments, were basically red clays and siliceous and calcareous oozes, globigerine ooze is what the calcareous oozes were often called. I think those terms still are used, still exist, but I tried to put them in some kind of quantitative way, with some sort of scientific criteria for them. Like, for example, the oxidation reduction potential, the so called Redox potential, whether the iron was reduced as in the blue muds and green muds or oxidized as in the red clays. I said that the pelagic sediments were always oxidized sediments and the terrigenous sediments were reduced sediments. There were some exceptions to this. For example, in the Amazon Basin, off the mouth of the Amazon--there was something called red mud which didn't have much organic matter in it but contained huge quantities of stuff carried down by the Amazon.

That was one of the interesting questions in those days--simply the classification of marine sediments. Nowadays, that's not a subject that anybody's interested in. They're interested in what's in the sediments, and how the components got there. Most of the terminology, I think, in sedimentation at least, has not changed very much. In physical oceanography they've introduced some new terms, like vorticity, for example, and if you asked me I wouldn't know quite how to define that. What it means is the curl, the turning of the water mass. One kind of current motion is called rotational and the other, irrotational motion. A water particle in a wave goes around in a circle but the particle itself doesn't rotate. It's irrotational, it has no vorticity. But if it goes around in a circle and rotates as it goes, then it would have vorticity.

Sharp: And they can measure it?

Revelle: Yes, and particularly in ocean currents. This turns out to be an extremely important property of ocean currents--everything now is in terms of vorticity. But if you push me much beyond that I can't tell you very much more about it. [laughs]

Sharp: Well, we don't have to go on at length about it but, especially in science, the terminology is something that is continually revised. The terminology does change and there are new words added.

Revelle: Yes, that's true. But, basically because of new kinds of observations usually based on new technology. For example, we never thought of heat flow from the earth's interior in the 1930s. The first man to think about it was Harold Jeffreys in his great book The Earth. But nobody thought about measuring it in the ocean until Teddy Bullard came along and decided it should be measured because Jeffreys thought it should be much less than in the continents. Similarly, we didn't think much about magnetic susceptibility or remanent magnetism, or magnetic properties of the sediments until a new technology was developed that could measure them. Many new phenomena were also discovered and they had to be named. For example we didn't think much about currents that later were called turbidity currents, which flowed out over the bottom because they were laden with sedimentary particles and therefore were heavier, so you actually had a flow of dense material along the bottom.

Sharp: Another layer practically.

Revelle: Yes. So the terminology basically reflects the new concepts, and the new observations, the new kinds of measurements.

It isn't like the ecologists who are always inventing new terms for the same thing. [laughs] Oceanography has been rather conservative in its terminology.

Sharp: Well, that makes less for me to learn, then.

Revelle: I don't think you need to worry about it.

One other early episode I recall is when I got involved with conflict about the Tijuana River Valley just north of the border with Baja California. The farmers in the Tijuana River Valley were saying that because there wasn't enough water coming down the river, they were getting salt water intrusion in their wells. I looked into this and wrote a paper on the criteria for judging whether salty ground water was sea water or not, or whether the salt was from some other source. That turned out to have been a widely read paper. I wrote it for the Transactions of the American Geophysical Union.

- Revelle: It was the first time anybody had ever thought about that particular problem. I was surprised to find, years and years later, that hydrologists were still referring to it.
- Sharp: You had done it as a very isolated sort of problem that they had brought to you.
- Revelle: Yes. There was a law suit in which Phil Swing, the great Imperial Valley congressman, was the attorney for the farmers and I was to testify for him. It turned out that the expert witness on the other side was Professor Ulysses Grant IV at UCLA. I was just an instructor and my future more or less depended on him. It was kind of scary.
- Sharp: Yes, I bet it was.##

IV OCEANOGRAPHIC RESPONSIBILITIES IN THE NAVY, 1941-1945

[Date of Interview: 27 January 1984]##

Sharp: I thought we would talk some about your work in the navy. One of the things I'm interested in is the wide range of scientific research that the navy was involved in during World War II, and then in the early post-war period. One of my first questions is why you went into the navy.

Revelle: Yes, the reason I was in the navy was--I told you about that yesterday--I got this reserve commission in 1936 because of the trip on the U.S.S. Bushnell. So I went on active duty in the summer of 1941 about six months before the war began, but when the UCDWR [University of California Department of War Research] started at the Navy Radio and Sound Laboratory in San Diego.* It has had a series of names since then. One was the Navy Electronics Laboratory and now it's called the Navy Undersea Systems Command. The first thing I got involved with when I came down there was being a project officer for some of the projects that the scientists were doing, particularly about that in "The Age

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indicate some of
the Papers, SIO, Box 14.

43a

May 1, 1941.

From: Lieutenant (jg) Roger R. Revelle D-V(S) USNR

To: The Commander Destroyer Division Fifty
(The Officer in Charge, Fleet Sound School, San Diego)

Via: The Commanding Officer, U.S.S. Rathburne.

Subject: Underwater Sound-Report on Sound Conditions in Pacific Area.

Enclosure (A): Surface Temperature Gradients and Predicted Sound Ranges in the North Pacific Ocean.

1. In connection with my period of active duty during February, 1941 a preliminary study of the basic factors underlying underwater sound conditions in the Pacific area was undertaken; this study has since been continued and a brief report has been prepared.
2. This report is submitted herewith as Enclosure (A) for consideration.
3. If it seems desirable to continue the study of the phenomena covered in the report I will be willing to undertake or to assist in such a study either on active duty or in a civilian status in connection with the activities of the National Defense Research Committee.

Roger R. Revelle,

May 20, 1948

Mr. Andrew Hamilton
Public Information
University of California
Los Angeles 24, California

Dear Mr. Hamilton:

Dr. Eckart has asked me to give you a brief summary of my activities during the past nearly seven years while I was away from The Scripps Institution.

I went on active duty in the U.S. Navy Reserve in July 1941 as one of the officers on the staff of U.S. Navy Radio and Sound Laboratory, San Diego, California, with the rank of Lieutenant (jg). While at this laboratory I worked on problems of Radar Propagation and Harbor Defense and organized a Radar Operator's School. During the latter part of this period I was also officer in charge of the Sonar Division of the laboratory. In December 1942 I was transferred to Washington as Technical Aide to the Hydrographer U.S. Navy, and Oceanographic Consultant to the Bureau of Ships and The Commander in Chief U.S. Fleet. My major duties in these various positions were to promote and organize oceanographic research in connection with problems of underwater sound, submarine warfare, amphibious operations and Air-Sea Rescue and to apply the results of this research to these fields of Naval warfare. I spent some time in 1945 in Manila on the staff of the Commander, Amphibious Forces, Pacific Fleet. In December 1945 I was placed in charge of the Oceanographic section of Joint Task Force One. This was the organization that conducted the Atom Bomb tests at Bikini. My duties involved planning, organizing and carrying out research on the geology, oceanography and biology of Bikini and neighboring atolls and measurement of the waves and seismic disturbances created by the explosions. In addition the Oceanographic section, which consisted of about 85 scientists and technicians, worked with Colonel Stafford L. Warren on problems of radiological safety. During my active service in the Navy I was twice commended; once for my war time work on military Oceanography and once for the work at Bikini. In November 1946 I became head of the Geophysics Branch Of The Office Of Naval Research, a position which I held until March 15, 1948. In the summer of 1947 I helped to organize a scientific resurvey of Bikini, which investigated the long term effects of the atom bomb explosions on the fish and other living organisms of the Atoll. I remained on active duty in the Naval Reserve until December 1947, during the last 3 years with the rank of Commander. I was later a civilian employee of the Navy Dept. with the rating of Head Geophysicist.

Roger Revelle

IN REPLY REFER TO NO.

44a

U. S. NAVY RADIO AND SOUND LABORATORY
SAN DIEGO, CALIFORNIA

2 November 1942

Dear Rawson:

Lieutenant E. K. Couper recently passed through Los Angeles on his way to Pearl Harbor and Dick Fleming and I took the opportunity of finding out all we could from him about the status of the Navy bathythermograph work and about my future job in connection therewith. Couper said that you are at least the presumptive father of the whole program and would be able to give me some much needed information.

As you probably know, I recently received orders to proceed when directed by the Commandant, Eleventh Naval District, to Washington in order to take up my new duties. The Commandant has forwarded these orders to the laboratory with instructions that Captain Hammond shall decide when I am to be detached here. Captain Hammond does not feel that he has all the necessary information to make this decision and has asked me to try to find out how soon I will be urgently needed in Washington.

The situation here is as follows: For the past several months I have been in charge of the work of the laboratory on harbor defense installations in the Eleventh, Twelfth, Thirteenth and Fourteenth Naval Districts under our assigned problem U54CD. We have felt that we should investigate and furnish help and information to the Districts on all technical problems connected with underwater detection devices used around harbor entrances. To this end we have carried out fairly comprehensive surveys of all the oceanographic and other factors we could think of which might affect the operation of these instruments. We have carried out three surveys in San Pedro, San Diego, and in Puget Sound and are now engaged in writing reports on the last two areas together with a supplementary report on San Pedro. We have been requested by the Commandants of the Twelfth and Fourteenth Districts to carry out similar surveys at San Francisco and Pearl Harbor.

Since Captain Hammond's arrival, the flood gates of

cooperation with the local NDRC group have been opened wide. In our next survey at San Francisco we are taking along a large group of NDRC people, both listening experts and oceanographers, not only in order to get the benefit of their various specialized abilities but also to give them experience in this type of work. Our idea is that we shall eventually have a team of scientists and engineers made up of about equal numbers of NDRC and Navy personnel and headed by one of our officers who shall be qualified to help with all phases of harbor defense installation wherever they are called upon to do so. The survey at San Francisco is beginning this Thursday and will probably take about a month. The same group will proceed shortly thereafter to Pearl Harbor.

Captain Hammond and I both feel that, if possible, I should spend some time at San Francisco in order to organize and instruct the group who will be working there. Also, I should remain here long enough to finish up our reports on the work previously done. Altogether, this will take about a month from the present date. I would like to go to Pearl Harbor for a day or so at the beginning of our survey there but this may not prove practicable. Another reason for going to Pearl Harbor would be to talk with Preston Mercer about the bathythermograph program and to see how Couper is getting along, but you are probably better able than I am to judge whether this is desirable.

After talking to Couper I feel very pleased about the opportunity to work in Washington and am naturally anxious to begin. As a matter of fact, my wife has already rented a house there and has had all our furniture packed and sent so that we are at present living in a rather primitive state in La Jolla. On the other hand, I am anxious to finish up my job here.

Do you have or would you be able to obtain any information on how soon I should arrive in Washington. This will probably mean that you should get in touch with the hydrographer and with somebody in the Bureau of Personnel. The information you send us will serve Captain Hammond as a basis for his decision as to when I should be detached from this Laboratory.

You are already so hard pressed by other duties that I

feel considerable hesitancy in asking you to help with this somewhat personal problem. On the other hand, you are one of those busy men who always seem to have plenty of time to help other people.

With kindest regards,

Very sincerely

Roger Revelle

Commander Rawson Bennett, U.S.N.
Radio and Sound Division
Bureau of Ships
Washington, D. C.

Revelle: Sound. That was a really uncomfortable experience because it rained all the time. It was perpetually cold and rainy. The last harbor entrance we looked at was the Golden Gate, the entrance to San Francisco Bay. That was a difficult problem because the peak tidal currents through the Golden Gate are about seven knots. You couldn't get a current meter below the surface, it just dragged out by the stern.

There was a group of physicists from NEL, Jeff Holter and Waldo Lyon and others, on that expedition. The commander of the Navy Electronics Laboratory was a man named Red Ruble (W.J. Ruble) who was a really terrible person. He just delighted in humiliating his officers in front of the enlisted men and then giving them impossible assignments. In general, he was a little tyrant. He was essentially so impossible to work with that I decided that I'd better get out of there. He didn't work very well at all with the UCDDR people. So they finally managed to get rid of him.

There was another captain named Hammond--he's in this letter, here--who worked very much better with the scientists in the UCDDR.

By that time I was committed to leave and I had written my friend Rawson Bennett with whom I'd worked before the war on underwater sound propagation. That letter of May, 1941 tells about the beginning of work on underwater sound. Well, then after that, Dick Fleming and Harald Sverdrup and I wrote a more comprehensive little paper on underwater sound propagation. Though we didn't know it Columbus Iselin and his group at Woods Hole had done the same thing and somewhat earlier than we had.

Anyhow, Rawson, who had been the sonar officer on the squadron of destroyers out here that were testing sonar gear went back to Washington as head of the underwater sound section of the Bureau of Ships. Pretty soon he was moved upstairs to the design branch of the Electronics Division--radio, radar and underwater sound, all the communications devices. He was a great big man. He had feet about the size of mine and was well over six feet tall. He was very heavy-set. He drank a great deal of coffee--didn't smoke however. He was a great leader. After the war he became head of this laboratory down here, which was by that time called the Navy Electronics Laboratory. He later became an admiral and Chief of Naval Research. He was the guy that I referred to in that "Innocence of War" paper when I described how we used to get Harald Sverdrup and Walter Munk cleared for security once a month, by walking down and intimidating the new Bureau's security officers. They had new security officers all the time in the Bureau of Ships.

I asked Rawson to get me out of San Diego and get me to Washington. And he did. I guess he had some limitation on personnel in his section, so he arranged for me to have a job in the Hydrographic Office.

Revelle: When I went to Washington, I reported to Admiral Bryan in the Hydrographic Office and also reported to Rawson. He'd arranged for me to have additional duty in the Bureau of Ships.

The Hydrographic Office was a hopeless place. It was entirely a charting outfit--making maps in that same old-fashioned way they'd been doing for a hundred years. Admiral Bryan, the only thing he could think of for an oceanographer to do was to look at some soundings that had been taken off Panama which showed that the bottom was quite shoal only a few fathoms or so deep. Nobody had ever been able to find this shoal since. It's quite clear, looking back on it, that it was the deep scattering layer that they were getting an echo from. As Martin Johnson showed, there was a layer of plankton organisms down at about a hundred fathoms in the daytime, which rose to the surface at night. The false sounding was the echo from these plankton organisms.

I went to Rawson and said I wanted to work with his group, so he gave me a desk in the Underwater Sound section. I became something called Code 940D which was the oceanographic subsection of the Underwater Sound Design section, of the Electronics Design division of the Bureau of Ships. It was from there that I did most of my oceanographic work during World War II.

My principal job was to be project officer for UCDWR, here in San Diego, the Woods Hole Oceanographic Institution, and the underwater sound propagation studies of a Navy laboratory called USNUSL, the US Navy Underwater Sound Laboratory at New London. Eventually I became project officer for all the various oceanographic contracts that the Bureau of Ships had.

Sharp: I want to stop you there. If you were to generalize about the oceanographic work that began to be done after you got to the Bureau of Ships, was it primarily involved in sonar and sound research and then, a real departure from what had been done before you got there? You mention the charting and the sort of old-fashioned--.

Revelle: Well, the Hydrographic Office, as I said, was primarily just a chart-making agency. The navy had never really taken an interest in oceanography until it was found that sonar propagation depended very largely on the temperature distribution in the upper layers of the ocean, the top one hundred meters of the water. This was useful in both submarine warfare and antisubmarine warfare. In anti-submarine warfare the sonar wouldn't work, if you had a sharp temperature gradient for more than a few hundred yards. In submarine warfare, the submarines looked for these sharp temperature gradients and hid under them, so that both the submarines and the destroyers had something called bathythermographs. One of my main jobs was to push these bathythermographs, to get them distributed

Revelle: and encourage the sonar officers, we called them bathythermograph officers, who went around peddling these things to destroyers. Some Woods Hole people, Allyn Vine and Alfred Redfield, designed a submarine bathythermograph which they installed on submarines. They were very popular with the submariners. We got many cruise reports from the submarines in the Pacific telling about how they used the bathythermograph.

Sharp: Was there resistance to using them?

Revelle: Not in the case of the submariners. Of course, it was a nuisance for the destroyers, but they used them quite a bit. We collected an enormous number of records particularly from the surface ships. The surface ship bathythermograph was a device that scratched a smoked slide. What it did was to measure temperature against depth. The temperature device was a bimetallic coil which coiled or uncoiled depending on the temperature. The pressure guage was simply a bourdon tube which compressed as it went below to higher and higher pressure, that is to deeper and deeper water. So, the scratch would go down like this because of the bourdon tube and over like this because of the bimetallic coil--the slide actually looked about like this and the trace looked like this. Usually there was a hysteresis, so the trace coming back looked maybe something like that. The up and down traces were very close and you could take a picture of them --you could blow them up to a size about so big with a grid on the negative. It would be depth this way and temperature this way. You could read them very well. You could actually read them right from the slide too, with that little grid to put back of the slide, you can find the depth of the thermocline, and the thickness of the mixed layer. This region in the thermocline was the place where the submarines could hide--right in the lower part of the thermocline. The other thing the submariners found which was perhaps even more useful was that because the density of the ocean water was increasing with depth, high density here and low density here since the water expanded when it was warmer, the submarines could sit here and not move. So they "sat on the layer" as they used to say.

Sharp: The impact of all of this, all of your reports coming in, that really showed you quite a bit about what--

Revelle: --The ocean was like.

Sharp: Yes.

Revelle: Sure, of course.

Revelle: It is still being used on merchant ships and navy ships. About 100,000 of what they now call XBTs a year are collected by ships of many nations. These underwater sound bathythermograph data were used in writing what we used to call "submarine supplements to the sailing directions." This was done by Mary Sears and her group at the Hydrographic Office. That's an interesting story in itself.

I came into my office in the Bureau of Ships one day. There was this short, heavy-set woman in the office who stood up as I came in [laughs]. I was, by that time I guess, a lieutenant commander and she was a jg. She'd just gotten out of the WAVE training school at Wellesley. She used to teach at Wellesley and when she put on a Navy uniform she'd gone back there to a navy training school. She'd been taught to stand up when a senior officer came in the room [laughs]. So she did it. I had really never known her although I knew who she was because she was on the staff of Woods Hole Oceanographic Institution.

So I took her out to the Hydrographic Office and introduced her to Admiral Bryan and asked if she could do the things that I was supposed to be doing at the Hydrographic Office [chuckles]. No one of course knew what those were. She got established out there and she built up an oceanographic unit in the Hydrographic Office with mostly biologists. Really, there were three or four biologists: Fenner Chase was one, Doris Henry was another. I don't remember the names of the others.

They started writing the "submarine supplements to the sailing directions" and various kinds of other intelligence reports. This was a whole new dimension for the Hydrographic Office. Of course, sailing directions had been issued for a long time. They dealt with tidal heights, tidal currents and bottom depths, primarily of harbors and shelters of various kinds. This program of Mary's just got bigger and bigger all during the war. In spite of her shyness and diffidence, she turned out to be a forceful leader--a very, very strong-minded woman.

Sharp: How long did she stay there?

Revelle: Just during the war, but that oceanographic division persisted after the war. Dick Fleming was in charge of it for a while. John Lyman was in charge of it for a long time. Those were both Scripps people.

Sharp: Did it begin to have its own evolution?

Revelle: It evolved to the point that the Hydrographic Office is now the Oceanographic Office. The hydrographer of the navy is now called the oceanographer of the navy.

Sharp: So it's in the right framework.

Revelle; [laughs] So this little thing that Mary Sears started, it is a whole goddamned office now! It's very interesting.

Usually the oceanographer is a Ph.D. in oceanography, usually a rear admiral; right how he's a captain because they couldn't find a rear admiral who had a Ph.D. The navy's meteorology and oceanography have pretty much combined now, I think. There's a big group up in Monterey [California] that make oceanic and meteorological forecasts.

One of the problems has been that in the Bureau of Ships, which is now called the Naval Ships Systems Command, the designers don't pay much attention to environmental conditions. They test their fancy new equipment always under conditions like this [gestures to view of calm ocean]. When it's foggy or when it's raining or when it's rough, they don't test them (at least that's what I've been told).

Then we got involved with a lot of other things too. One of my jobs in section 940D came because I was project officer out here for the UCDWR [University of California Department of War Research]. One of the groups here was designing some mine detecting gear for submarines using what they called frequency modulated sonar. This was fairly high frequency sound about I guess 30,000Hz --30 kHz. This frequency was primarily useful over short ranges. The problem was to get through the Straits of Tsushima. The Japanese had mined the straits, the entrance to the Sea of Japan and we'd lost two or three submarines trying to get through. So the submarine force decided that they wouldn't try to get through this mine field until we had mine detecting gear. We finally got the mine detecting gear on the submarines in the summer of 1945. Twelve submarines went through the straits; only eleven of them came back. One was sunk by our own mines.

We, by that time, had essentially destroyed Japanese shipping in the Sea of Japan by planting what were called oyster mines or pressure mines. The mines were developed in the Bureau of Ordnance; it had nothing to do with the Bureau of Ships. Charlie Fish of the University of Rhode Island was also in a sailor suit and he, I guess had something to do with the development of the mines. A man named Ellis Johnson who was from the Department of Terrestrial Magnetism of the Carnegie Institution in Washington did also. The Naval Ordnance Laboratory designed these pressure mines--Charlie Fish did not design them. They were mines that were activated by the pressure signature of a passing ship and they were essentially impossible to sweep except with a ship. So they're very difficult things to handle. Our 29th bomber command--it was called the "29th bomb comm"--with Ellis Johnson as project officer had dropped these things in all the Japanese and Manchurian harbors. As a result there was essentially no Japanese shipping across the Sea of Japan

Revelle: from Manchuria to Japan. Our twelve submarines managed to sink something like 25,000 tons of Japanese shipping and lost a submarine in the process. That was not really a very good payoff.

But that's the way war is. It's a wasteful, terrible business and all of our work, by the time we got them on, the mine detectors on the submarines didn't amount to very much. We got the mine detecting gear on--

Sharp: Very late in the game.

Revelle: That's right. But we got involved with various other things. Walter Munk and Harald Sverdrup [telephone interruption] had worked for an Army reserve officer named Richard Seiwel who was a Woods Hole man on active duty in the Army on forecasting of sea and swell, basically forecasting of breakers, surf forecasting for amphibious landings. They came back here and started this. They both came back to Scripps, not to NEL because they had clearance problems.

Sharp: Because they were immigrants?

Revelle: That was I guess the principal reason. They were both foreign born. Norway was of course under German control and so was Austria. I said in "Innocence and War,"* Rawson and I had to get them cleared about once a month. They were always having trouble.

One interesting sequel is that Walter Munk is now the darling of the navy. He's learned so much about underwater sound that at his sixtieth birthday party the chief of naval research came out to help celebrate it.

Sharp: Because he's really one of the key scientists in terms of understanding what they need to know.

Revelle: That's right.

Sharp: It sounds like the relationship between scientists in uniform as well as civilian scientists during the war really made an about-face, or at least started to because of the increased use of the scientific--

Revelle: Oh yes! From the standpoint of government support of science, there was a tremendous change during World War II not just in oceanography, but in many many fields in science.

*See footnote on page 44.

Revelle: Some other things that we did, one of them was to study the drift of life rafts under the combined action of wind and currents. We produced handkerchiefs that aviators could carry, waterproof handkerchiefs. They're really sort of scarves that show the currents and the location of islands where they were flying. So if they went down they could at least have some sense of where they were and what they might do. These were designed by Harald Sverdrup.

Another thing we did was to study the behavior of smoke. This was really an idea of a guy at Woods Hole named Al Woodcock who's now out at Hawaii. He had noticed that there are zones of convergence and divergence in the surface currents of the ocean. The regions of convergence would be regions where the water would be sinking; therefore it would be warm and smoke would rise. Regions of divergence would be where the water was cold and where the air near the surface would be cold, and the smoke would stay near the surface.

We had a whole group led by Jeffries Wyman of Harvard and one of my associates Cesare Lombardi Barber who was an English instructor at Amherst. He got involved with the smoke business. He was always called Joe Barber, but his real name was Cesare Lombardi Barber, one of these names that mothers invent. He later became the smoke officer for the Pacific fleet and it turned out that they used smoke in quite a different way than we had in our tests. Smoke was used primarily to defend against kamikazes. The Bureau of Ships provided smoke generators for all the ships that were involved with the landing operations, mostly transports, of course, and cargo ships. Whenever there was danger of a kamikaze attack, they would start these smoke generators and make a blanket of smoke over the whole fleet. Well, the poor kamikazes didn't know where to go.

Sharp: That was pretty effective then?

Revelle: That was quite effective, yes. Joe Barber finally ended up teaching English at Sanga Cruz, UC Santa Cruz. He died about seven or eight years ago of cancer. He was a wonderful man.

Another research project was mainly carried on by Murrough P. O'Brien, J.W. Johnson, John Isaacs and their colleagues at Berkeley. O'Brien was dean of the College of Engineering there. You may remember the Tarawa landing in the Pacific? This was a landing on an atoll in which the shallow waters of the reef grounded the landing craft about a mile from shore and the Marines had to wade across the reef, sometimes over their heads, sometimes with just two or three feet of water under them all the way to the beach. They were slaughtered by the Japanese. We lost something like two or three thousand Marines. After this experience the navy decided we had to

evelle: have better intelligence about water depths off a landing beach. One way to get this information was to use the underwater demolition teams. Another was to use the general philosophy of the waves coming into the beach. I'll show you how it works. These waves are coming in more or less parallel to the shore like that. If the offshore topography of the beach looked like this with here a canyon, and here, a ridge, the waves would look pretty much the same. They would go faster here where the water is deeper and slower where the water is shallow. So the waves follow the contours of the bottom. You can take pictures of this from an airplane and get a pretty good idea what the bottom depth is. The waves shorten up as they come into shallow water and they steepen up. You can watch them right here through our window. You can hardly see the waves a mile off shore there, but as they come into the beach they get steeper and the distance between them gets shorter and then they finally break when they "feel" the bottom as the saying goes. They're feeling the bottom whenever they peak up. But they feel it more and more and finally they break. Just now the bottom topography is flat and gently sloping so the waves are coming in parallel to the shore and they don't bend. When the line of waves does bend as they're coming in, here, where they peak up or where they're held back, then you get an intensification of the wave energy. Here, where they run ahead, the wave energy stretches over a longer distance. So the waves are low here and relatively high here. That could be used in amphibious operations too. You'd want to go in on this place where the waves are relatively low, not on this place here.

O'Brian and his colleagues developed this technique of measuring the depth of the bottom from the waves to a rather refined degree. I spent the summer of '45 in the Pacific basically to teach the intelligence officers about this technique. I went out there also to see about our submarine mine detecting gear and how it was working.

I ended up in Admiral Raymond Spruance's staff in Guam. They were planning for two major operations in the invasion of Japan. One was called Coronet and one was called Olympic. Olympic was to be a landing on Kyushu, on the southern island. Coronet, as I remember it, I may have the names reversed, planned to land on the east coast of Honshu, the big island, Honshu, near Tokyo. Admiral Spruance was very unhappy about both of these proposed operations. He was afraid millions of people would be killed on both sides, particularly on the Japanese side. He just hated it. I don't know whether he knew about the atom bomb or not. Maybe Admiral Nimitz knew and Spruance didn't. I remember that he asked me to go see Nimitz who was then CINCPAC, CINCPAC, commander-in-chief of the Pacific Fleet and of the Pacific Ocean area. He was the chief military officer in the central and northern Pacific in charge of all our forces. There was also a Southwest Pacific commander, General [Douglas] MacArthur was in command then.

Sharp: The amphibious landing that was considered, was that as an alternative to the bomb and then, the decision was made?

Revelle: If so it certainly was not at the level of most people out at Guam. They didn't know about the bomb. Nobody knew whether the bomb was going to work until a few weeks before they actually used it, when they tested it at Almagordo in New Mexico. At the same time they were planning these amphibious operations. All the previous amphibious operations had been very hard, difficult things, every one of them.

I remember Admiral Spruance sent me to see Admiral Nimitz to tell him about this method of measuring depths on the beaches. The other way to do it (they didn't rely just on our way) was to use underwater demolition teams. These were people equipped basically with snorkels (they didn't have scubas in those days), at night they would swim in and take soundings, a very, very dangerous business because there were lots of traps, booby traps of various kinds. They'd try to locate all these booby traps.

I explained this business to Admiral Nimitz, but I didn't get much response from him at all.

In fact he was the most intimidating guy I ever met, at that time, frosty blue eyes. I remember going into his outer office seeing the chief of staff. The chief of staff said, "You go on and sit in this other room and Admiral Nimitz will send for you." So after I'd sat there for about half an hour the marine guard came in, clicked his heels, looked straight at the ceiling and said, "The fleet admiral will see you now." I went into talk to this cold looking man with his icy blue eyes and mumbled and stumbled [laughs].

Sharp: Did you make sense? Did you explain--.

Revelle: I explained it all right I guess, but he must have know about the bomb because he wasn't very much interested. Later, we became very good friends. He became a regent of the University of California and our marine facility down here is named the Nimitz Marine Facility for the very good reason that he was a loyal and effective regent on the side of the Scripps Institution of Oceanography and, of course, on my side as its director.

In any case, Admiral Spruance then sent me to Manila to join the staff of Admiral Redmond Kelly Turner, the commander of the amphibious force who was actually going to carry out the operation. In the instant that I climbed the ladder of Eldorado, which was Admiral Kelly Turner's flagship, the news came over the loudspeaker that they had dropped an atomic bomb on Hiroshima. That was in reality the end of the war right there, and Admiral Turner knew it. From

Revelle: then on we really did very little. I stayed out there on the Eldorado for about a month or so. The army people were on the beach and it was very interesting to see the difference between the navy and the army: the navy living in style on its ship and the army living in squalor on the beach.

Manila was a shambles; it had been almost completely destroyed by the bombing and shelling during the American invasion. It's amazing to go out there now and see how those Asian cities have come back.

After the other atomic bomb was dropped and after the Japanese surrendered, I went back to Washington. By that time the National Defense Research Committee [NDRC] had essentially disappeared. Lyman Spitzer had gone back to teaching at Yale. During the war he ran something called the sonar analysis section of the underwater sound division of the NDRC, and we worked very closely together. Lyman was trained as an astronomer, but he's basically a theoretical physicist and knows a lot of mathematics, and is very, very bright.

After the war there were two things that he did. He was the first person to propose an orbiting astronomical observatory. Then he got involved with the atomic fusion project, the development of thermonuclear power and he ran the project at Princeton for many years. We were very good friends. He was somewhat younger than I was. God knows I was young enough. In 1944 I was thirty-five years old.

For several months after the surrender of Germany, and before the surrender of Japan, the NDRC was getting out of business. It was up to the Bureau of Ships to take over the things that we wanted to keep on going. That was one of my jobs, to arrange that.

Lyman and I wrote a letter for the chief of the Bureau of Ships to sign, setting up something called the Marine Physical Laboratory at Point Loma [California] which would be led by Carl Eckart, the great physicist from the University of Chicago. This was an interesting experience because the navy had never supported university-based research before--although it did support basic research at the Naval Research Laboratory. Moreover, they didn't like to make long-term commitments. Carl Eckart insisted that they couldn't just give him a year-to-year contract. It had to be a more or less permanent arrangement. I remember Admiral Cochrane couldn't make up his mind about this for quite a long time as you can imagine [chuckles]. We kept pushing him and finally he signed the letter that we had written which established the Marine Physical Laboratory. Lyman was very much involved with that as well as I.

Sharp: I want to stop you and ask you a few other questions because I'm not sure how these other activities tie in, but they're in this same time period. What about the National Research Council?

Revelle: The National Research Council was formed during World War I as an operating arm in the National Academy of Sciences. It had almost nothing to do with military research during World War II. President [Franklin D.] Roosevelt set up another organization, just as [Woodrow] Wilson set up the National Research Council. Roosevelt set up the Office of Scientific Research and Development (OSRD) which had at least two branches and maybe three. One was the Committee on Medical Research and another was the National Defense Research Committee (NDRC). In some way, they also were involved with the early days of the atomic bomb development. Certainly Bush was, Vannevar Bush. He was the head of OSRD and President James Brian Conant of Harvard was head of NDRC. They were the people who guided the early part of the atomic bomb development too.

Sharp: It seems that there was also then the Naval Research Advisory Committee.

Revelle: There was also a post-war thing. What existed during the war, from the standpoint of military research, was the NDRC. But they were abolished at the end of the war. The executive officer of the OSRD was a young man named Carroll Wilson who had been at MIT and was picked by Bush. He later became the executive officer of the Atomic Energy Commission. Eventually he went back to MIT as a professor in the Sloan School and he died about a year or so ago. He was a great scientific entrepreneur, Carroll Wilson was.

The sort of ended the war period. We set up the Marine Physical Laboratory to carry on the underwater sound research which had been done during the war by NDRC. At the same time I was assigned to the Office of Research and Inventions as head of the geophysics branch.

Sharp: That's where ONR came out of.

Revelle: That's right. It was the first name for ONR.

V OPERATION CROSSROADS, 1946

Preparation and Conduct of Tests Able and Baker

Revelle: I stayed on at the Bureau of Ships and didn't really do much for the ORI for about a year. The reason was that at that time they were planning to test atomic bombs against naval ships. That was the origin of the Crossroads operation.* Mary Sears and I were approached by two young officers who were on Admiral Parson's staff.

Sharp: Ashworth and Rivero?

Revelle: Yes, exactly. Fred Ashworth and Horatio Rivero. I have no idea what happened to Ashworth, but they both became admirals eventually. Admiral Rivero became vice-chief of naval operations and later became commander of naval forces in Europe, at least in southern Europe. He was the supreme commander of American forces--Allied forces in fact, NATO forces. He's now retired. He's a wonderful

*For additional information on Crossroads, interested readers may see "Bikini--A Lost Way of Life," William S. Ellis, National Geographic 169 (No. 6, June 1986), pp. 813-834; and, "The 1946 Atomic Bomb Tests: Atomic Diplomacy or Bureaucratic Infighting," Lloyd J. Graybar, Journal of American History, 72 (No. 4, March 1986), pp. 888-907.

Revelle: little man. He's a Puerto Rican, quite short, and just as smart as he can possibly be--a wonderful person. That's often the case with admirals; not always, but often they're very bright. He and Ashworth were the staff officers for Admiral Parsons and the first problem was to find a place to hold the test.

Sharp: What were the criteria for the test in terms of finding a place for it?

Revelle: The criteria was to find an island or an atoll in the Pacific, a remote land base in the Pacific where the effects of the bomb would presumably, the radiation from the bomb, do the least damage. We picked Bikini atoll as a remote place in the Marshall Islands. I don't remember just who suggested it, but Mary Sears and I thought that was a good place to have it. It really wasn't an awfully good place, but there aren't any awfully good places [chuckles].

What they wanted to have was enough land so that they could set up the laboratories and the equipment. They wanted it to be as calm as possible, so they could fly sea planes in and out. They wanted it, if possible, to be free of hurricanes which meant you couldn't go too far into the western Pacific.

They wanted to have as few people displaced as possible. We didn't really know how many people there were on Bikini atoll, but it was necessary to move them all off and they were moved to an island called Rongelap, an atoll about 60 miles to the east. This was certainly not a very good thing to do because the atomic radiation from the cloud drifted over Rongelap and some of those people got some exposure to radioactivity.

Sharp: What were the planning sessions like for the operation?

Revelle: Oh, we had them every day. We had a staff meeting every day. I became the oceanographer on Admiral Blandy's staff. He was the commander of what was called Joint Task Force One, JTF1, which was a combined task force of the army, navy and air force. By that time there was a separate air force. Crossroads was planned in the most minute detail in Washington. It was very interesting to me to sit in on those meetings.

By that time I was a commander which was still a pretty low rank in Washington, although it was pretty high in the Pacific. You get out to Guam as a commander, you get a lot of kowtowing. In Washington, commanders were a dime a dozen, about the lowest rank that had much to do with anything.

Anyhow, I sat in on all these staff meetings and it was very interesting because of how it was run. There was no such thing as voting. Admiral Blandy did all the voting [laughs], but he was so good at it that he would find a consensus every time or nearly

Revelle: all the time. So everybody agreed with his decision. It was really quite remarkable to see how he operated. I gained a great deal of respect for him. His name was William Henry Purmort Blandy; he was always known as Spike Blandy because he had a big nose, a big pointed nose. Later I looked up his record at Annapolis and--

Sharp: Impressive?

Revelle: He was the number one man in his class in Annapolis just as I would have expected him to be. He could argue with the meteorologists, for example, on their own grounds--he didn't really know much about meteorology at all, but just by sheer logic and sheer penetrating rationality he could catch them up in inconsistencies and uncertainties. It was just because he was smarter than they were. He was smarter than anybody in the task force. They had a civilian in charge of the scientific aspects of the test--measurements of various kinds--named Ralph Sawyer who later became dean of the graduate school of the University of Michigan, the Horace H. Rackham graduate school. He and Ashworth and Rivero all were the senior staff of Admiral William S. "Deke" Parsons. Parsons had been very much involved with the atomic bomb development and he was the bombardier on the Hiroshima plane. He dropped the first bomb as a captain I guess, maybe even a commander. He was quite a young admiral.

Finally after about a year of planning everybody moved out to Bikini. But I had gotten things going before then as far as oceanography was concerned because we thought we ought to make an ecological survey of the atoll.

Sharp: I've seen a sort of timetable for the operation, and I guess January of '46 was the official beginning. But you had been there three months before?

Revelle: I don't know. I don't remember exactly the day. Our people were there several months in advance of anybody else.

Sharp: About these planning sessions, was it entirely obvious what tests needed to be made or was it a matter of making decisions among the scientific team--.

Revelle: We had to do a lot of thinking about it. It wasn't obvious at all. Oceanographically, we thought that there were really three problems, as I remember it. One was to measure the waves that were created by both the air drop bomb and the underwater bomb. There were lots of quite different ideas, diverse ideas about how big those waves would be. The second problem was to follow the radioactivity, both inside and outside the atoll, the diffusion of that radioactivity. The third problem was the effects on the organisms and the atoll itself, on the coral reef itself, of the explosions.

Revelle: We did a lot of work on all three of those problems. To give you some idea of the complexity of it, in measuring the waves we set up three photographic towers, one-hundred-foot towers, with automatic cameras on them. The pictures that you've seen, particularly of the underwater test, that huge base surge--those were all taken by those cameras of ours.*

Sharp: John Isaacs had directed all of that, I suppose.

Revelle: He was the one who really pushed those towers and automatic cameras across, along with a man named Alexander Forbes, who had gotten himself to be a lieutenant commander and was very much interested in photography. He was a member of the famous Forbes family of Boston, and deaf as a post. In World War I he had turned his yacht over to the navy and had commanded this yacht in anti-submarine patrolling off the East Coast. He was a character right out of Santayana's The Last Puritan. Very nice man, but as I said quite deaf. He was also quite fearless--and not very physically apt, so we tried our damndest to keep him from climbing those towers, which he insisted on doing. We were always afraid he was going to fall down and break his neck. Anyhow, he did climb them practically every day. He must have been close to seventy at that time. He was a physiologist; he taught at Harvard.

The second way we did it was an idea of Jeff Holter's, who had been my assistant at the Bureau of Ships. His way, I think, was by far the most ingenious. He set up a series of poles in shallow water with tin cans on them, and the tin cans were open at the top.

Sharp: To catch the water.

Revelle: The cans were just one foot apart all the way up the pole, and the cans that were filled with water meant that the wave had broken over them. The cans that just had a little spray in them were above the height of the wave. So you could get the height of the wave within one foot, at least, with these cans. Wonderfully simple.

Sharp: Inexpensive.

Revelle: Relatively inexpensive device.

*See following pages which are Science Service Wire Reports of the Bikini tests in 1946. Revelle Papers, SIO, Box 14.

SCIENCE SERVICE WIRE REPORT

By DANIEL WILKES
Science Service Crossroads Correspondent

7/24/46

ABOARD USS APPALACHIAN AT BIKINI, July 24 - What a well aimed atomic bomb will do to a capital ship will probably be answered in this outdoor laboratory in the second Bikini test today.

The aircraft ^{carrier} ~~Saratoga~~ and the battleship Arkansas are the closest capital ships to the empty ring of water about half a mile in diameter in which the bomb will be detonated. The exact position of the bomb within the circle has not been disclosed.

If the underwater blast demolishes these ships or puts them out of action unequivocally, a big question in naval warfare will be answered, at least partially and so far as old capital ships are concerned.

An inspection trip around Bikini lagoon viewing effects of the first blast shows that the destructive power of the atomic bomb is not unlimited. It has a fairly well defined radius of destruction but within this radius of about a half mile nothing can withstand it. Big ships will be within this radius in the second blast. They will be subjected to the triple threat of concussion, flash and deadly radiations.

Twenty pigs and 200 rats are substituting for the crew aboard the ships of the second test. Capt. R. H. Draeger, USN, in charge of animal medical tests, revealed to me. As these are placed as the crew would be stationed in action, many of these animals are without doubt in the interior of the ships.

Two destroyers equipped with Geiger counter apparatus are ready to pounce upon the ocean currents that carry the radioactive products created by the atomic bomb underwater explosion and trace them wherever they may go.

Comdr. Roger Revelle, on leave from the Scripps Institution of Oceanography, La Jolla, Calif., is the chief oceanographer who will use the fission products and the transmuted sodium in the salt of the sea water for putting a finger on the currents which will carry radioactivity out of the lagoon to the open sea.

The two destroyers can take samples of sea water at great depths. This makes it possible to determine the interchange of water between the surface and the depths as well as the direction of the currents. The scientists on Comdr. Revelle's staff are ready to extend this world's largest radioactive tracer study to wherever the radioactive water carries them, no matter how far at sea.

By SCIENCE SERVICE

7/24/46

WASHINGTON, July 24--Radio reports of the atomic bomb test may not get through from Bikini this afternoon because of radiations from the sun.

Shortwave broadcasts may be expected to fade out suddenly during daylight
(more)

WIRE REPORT - Sheet 2

7/24/46

hours for the next 10 days, the National Bureau of Standards warns. Such blackouts are due to eruptions on the sun that in size would completely dwarf atomic bomb explosions,

These blackouts usually last for an hour or two, but the exact time they will occur cannot be predicted. When they take place, all frequencies fade out within a minute.

Shortwave broadcasts, particularly those following North Atlantic paths, are expected to be disturbed from Thursday through next Tuesday, with severe blackouts expected Saturday and Sunday.

By DR. FRANK THONE
Science Service Staff Writer

7/24/46

WASHINGTON, July 24 -- Radioactivity from the explosion of the submerged atom bomb, scheduled for this afternoon, should affect several chemical elements abundant in Bikini lagoon, in addition to the hydrogen and oxygen of its water, and the sodium and chlorine of its salt.

They are contained in the fine silt on the bottom, which will be roiled up by the tremendous disturbance and which will probably take many days to settle down again.

Most abundant of these elements are calcium, silicon and carbon, each of which is known to form several radioactive isotopes. The calcium comes from ground-up coral and seashells and from the skeletons of microscopic one-celled animals. The silicon is in the shells of diatoms, which are one-celled plants that form the basic food of all marine animals--often called "the grass of the sea."

The carbon is the fundamental element in the flesh and fat of all living things, from microscopic forms to the biggest sharks. There will be wholesale death in the lagoon waters when the bomb explodes, and the radioactive debris of this rending will lurk in the water for an unknown length of time.

By DANIEL WILKES
Science Service Crossroads Correspondent

7/24/46

ABOARD USS APPALACHIAN, BIKINI ATOLL, July 24 -- Darwin was right.

His century-old theory, that coral atolls were formed by the growth of coral on top of slowly submerging volcanoes, has been given positive support by seismographs that recorded the movement of artificial earthquake waves started by the first atom-bomb explosion, on July 1. Dr. R. M. Tripp, seismologist with Joint Task Force No. 1, states that these waves were reflected in a pattern showing that the atoll consists of a mass of coral 7,000 or 8,000 feet thick, on top of a solid mass of rough rock.

A rival theory, that atolls were built by coral growth on top of flat submarine platforms, is apparently knocked out. The seismic waves did not come back up as they would have if they had been reflected from a flat surface of underlying rock.

- Revelle: The third thing we did was to design a series of what we called turtles, which were about so big and so high (about two feet in diameter and six inches high). These were pressure measuring instruments which we planted on the bottom of Bikini Lagoon. They would tell you the change in the height of the water above them, which was reflected in the change in pressure on the bottom. The pressure, of course, of the explosion itself was a very sharp, sudden thing, whereas a wave would be slow and gradual. They were damped so that they would detect only the slower change. They worked pretty well. Allyn Vince designed those.
- Sharp: I've seen a note that there was something like ten thousand instruments.*
- Revelle: Were there? Certainly not that many oceanographic instruments.##
- Sharp: There was a sort of a photo history which must have used a lot of Isaacs's photographs but put together by a historian of the operation. This person is never named; I don't know who he or she was. But there are descriptions of the tests and quite a few photographs of the burst and of the base surge, this 1000-foot high base surge, and of some of the instruments and of the photo towers that were used and so on. Anyway, one of the statements was that ten thousand instruments were used, were put either on the target ships themselves or--.
- Revelle: Well, that may very well be, because they measured a lot of different things. One of the things they measured was the pressure, of course, in the air, the air pressure as well as the underwater pressure. There were various devices for doing that. Again, a very ingenious and simple one was designed by Bill Penney, who later became Lord Penney. He was my roommate on the scientific ship that I was on. Do you remember the name of that ship? I've forgotten it for the moment.
- Sharp: No.

*Operation Crossroads, The Official Pictorial Record, prepared by the office of The Historian, Historian of Operation Crossroads. (New York: William H. Wise & Co., Inc., 1946), p. 9.

Revelle: I remember--it was Kenneth Whiting. Bill Penney and I shared a room on that ship. His ingenious device for measuring the pressure was to, again, just have cans, sealed cans. How much they were crushed was a direct measurement of the peak pressure. It didn't give you the pressure signature, but it gave you the peak pressure. Again, very, very simple, cheap, ingenious devices that depended on a lot of high-powered thought. Those were the best kinds of instruments. A great deal of insight goes into their design, and the end result is something very simple.

Sharp: What do you remember about the different observers who came to watch the tests? I had read that there were some United Nations observers. I wondered if you might have had contact with them. There were also some people who came from the Soviet Union, I understand.

Revelle: If so, I didn't know about them. I don't remember them.

Sharp: The Panamint was the ship provided primarily for the observers.

Revelle: The only people that I remember as observers were congressmen and reporters. There were a lot of both reporters and congressmen. I don't remember any foreign observers. They may very well have been there though.

For the actual test itself we got on Admiral Blandy's flagship, Mount McKinley. There was one ship the U.S.S. Albemarle, I remember, which was called "the Able Mable."

Sharp: These Science Service wire reports were written off the U.S.S. Appalachian.

Revelle: That was some kind of a transport.

Anyhow, we observed both tests from Admiral Blandy's flagship. All of the ships got out of the lagoon, of course, except the target ships. I was standing next to Norris Bradbury, who was my classmate at Pomona College and by this time had become director of the Los Alamos Laboratory, succeeding Bob Oppenheimer. I remember that when the first bomb was dropped, the air drop bomb, it went off. Norris was beside himself. He jumped up and down; he said, "Those things always go off!" He was very, very pleased that it had worked.

The Los Alamos people had a lab on Eniwetok, which was an atoll just to the west of Bikini. They did all the preparations there; those were all quite secret. I was on Eniwetok a few times, but I never was there very much.

Revelle: We did a lot of work on seismic exploration of the atoll, too, to try to find how thick the coral was by getting seismic reflections. A man named Beauregard Perkins was very much involved with that. He was a geophysicist. With a name like Beauregard, you can imagine he was from the South. He was one of my group.

Altogether there must have been a thousand people in our oceanographic group, counting all the enlisted men involved. We had a special ship, the Fulton, with a real son of a bitch for a captain, named Captain Sanangelo, who took a dim view of the scientists.

Sharp: That's the wrong sort of captain to get for that.

Revelle: Yes, that's right. But it didn't matter very much because we were always frustrating him.

Sharp: Do you remember what you thought as the air burst went off?

Revelle: Well, I thought it was a big bang. Horrible sight.

But nothing like the underwater burst. The underwater burst was a great surprise to everybody, although it shouldn't have been. We had conducted some model tests in Chesapeake Bay before the tests.

Sharp: Those were the TNT tests?

Revelle: Yes. Mike O'Brien was in charge of that, Dean [Morrough] O'Brien at [UC] Berkeley. Of course, the way to see what happened was to take pictures, a series of high speed photographs. If you look carefully at those photographs you'll see a base surge, you see a kind of mount of water--what looks like water but is really a spray--moving out. But we never noticed that. So when the underwater explosion went off, there was this huge pillar of water, really just mostly spray, that went up into the air. You couldn't really tell what it was, but it looked like a great, huge column of water shooting up into the air. Then it settled down again and went out in this base surge.

The base surge, as I remember it, was several hundred feet high at first and moving very fast. We thought it was a huge wave. In other words, that our estimates of the waves had been completely haywire. But it moved more and more slowly and after a while it just stopped. Then after a while it lifted from the water altogether. All it was was spray, a lot of spray; but of course it rained out, and as it rained it became lighter. Finally it got so light that it just lifted from the surface altogether.

Revelle: This was a real phenomenon, this spray. It probably had a lot to do with covering a lot of the ships with radioactive material.

Sharp: Because it just came down on them.

Revelle: Yes. It spread out over an area of about a mile, as I remember it. So a lot of the ships were covered with radioactive water. The principal unexpected problem was the radioactive contamination of the ships. They tried their best to what they called "decontaminate" them.

Sharp: By flushing?

Revelle: Yes. They tried all kinds of things. The man in charge of the damage surveys, the damage control and rehabilitation, was a man named Admiral Solberg, Torvald Solberg, who had been chief of the research division of the Bureau of Ships. A very nice man, and a good friend of ours.

I remember at that time Ellen and I and our children had moved to a house in Washington just off Foxhall Road. I remember we gave a party there one night, a cocktail party. Admiral Solberg was among others invited, and he was quite late in showing up. I remember my daughter said, "Daddy, you promised me three admirals, and there's only two admirals here!" (Solberg wasn't there yet.)

They later established a laboratory at San Francisco at Hunter's Point, the Navy Radiation Laboratory, which was designed basically to find ways of decontaminating navy ships that had been covered with radioactivity. Quite a job.

Impacts of the Tests and Lessons Learned

Sharp: Can you make any general comments about the impact of the tests of Able and Baker on the scientific community? Do you have any sense of what their reaction was to the tests and the results of the tests, all the measurements that were made and all the observations?

Revelle: I don't think you can make a very simple statement about that. The interesting thing was, biologically, that the tests had very little effect on the atoll. I organized a resurvey of Bikini.

Sharp: In '47?

Revelle: Forty-seven, to go out and see what had happened. It was quite clear even then that the effects of man on the atoll were ephemeral.

Revelle: All of our buildings were decaying already, and our awful, big masses of equipment were just gradually rusting away. The organisms didn't seem to be bothered much at all. However, there was still a lot of radioactivity there, and it was quite dangerous. We didn't realize how dangerous it was [for human beings].

It was thought for a while, not so much by the scientists as by the military, and particularly by the air force, that this was the end of the old ships, that the bombs were so powerful and actually could be used over such a wide area, that surface ships really didn't have much chance. But that opinion did not persist.

What it did mean, however, was you had to disperse your fleet; you couldn't get them too close together.

Everything else worked in the same direction, more and more action at a distance: the missiles, for example, and rockets, weapons of various kinds that have a range of fifty miles or thereabouts, homing torpedoes that could be launched from great distances. That's the way the navy has gone in the last thirty years or so.

The oceanographers at Bikini, I think, enjoyed working together, enjoyed the interdisciplinary approach to the problems. Ken Emery, a geologist, was there, for example; he surveyed Sylvania Seamount, which was a seamount right next to the atoll. There was a man named William Randolph Taylor, who studied the algae. We had some professional fishermen who caught fish both in and outside the lagoon. Jack [John C.] Marr, who was later head of the U.S. Fish and Wildlife Laboratory here in La Jolla, was in charge of the fishing part of it.

Sharp: That was for the biological survey?

Revelle: Yes. They were all on the Bowditch, these people. The Bowditch stayed throughout the entire operation.

I don't think there was any revulsion against the bomb at that time, at least not much, among the people out there. There was a lot of revulsion back here, particularly led by Leo Szilard. They organized at that time the Federation of American Scientists, and they started the Bulletin of the Atomic Scientists, with their famous clock and its hands pointing to just a few minutes before midnight. Szilard and the scientists who had been involved with the bomb started this organization--most of us didn't even know the development of the bomb was being done; the Manhattan Project had the best security that you could possibly imagine.

Revelle: I remember when these people left San Diego, the physicists from Berkeley, and went off somewhere to a place called Shangri La, I thought it was just a huge boondoggle. So it was a great surprise to find that they'd made it work. There were a lot of people involved with it, but they were all isolated from the rest of us, certainly from the oceanographers.

But after Bikini, Scripps Institution took part in several other tests out there.

Sharp: That was one of the questions that I had, because there were quite a few examples; two other ones that I saw notes about were Ivy, which was in '52, and Wigwam, which was in '54.

Revelle: Wigwam was an underwater test off the Mexican coast.

Sharp: My question with respect to those was asking for some comments about the history of Scripps's involvement in these tests, and how you would describe it.

Revelle: We always had the same job, which was to measure the waves and to look at the diffusion of the radioactivity. John Isaacs was very much involved with that; Bill [Willard] Bascom was very much involved; and Bill [William G.] Van Dorn with the problem of possible tsunamis on distant islands. Bill Van Dorn set up instruments to measure waves at a distance from the explosion. We thought there might be tsunamis set up by these big explosions.

There is a very amusing story there. John Isaacs and Walter Munk and I thought there might be a huge landslide as a result of the hydrogen bomb test at Bikini in 1952. The coral reef is a very vertical sort of thing; it's almost overhanging, with steep vertical sides. It looked as if there had been previous landslides; that is, it looked as if big pieces of the reef had fallen off in the past, although the evidence wasn't very good. But we thought that this might happen with the hydrogen bomb test, a far larger explosion than the nominal atomic bombs at Crossroads.

We persuaded the navy that this was a real danger, a really possible hazard. They set up a warning system for the other islands in the Pacific to get people off of them if necessary. They consigned a lot of the instruments to aircraft so they wouldn't be affected if there was a big tsunami. We set up some pressure measuring devices outside the atoll. They were suspended from buoys moored in fairly shallow water outside the atoll.

We had Walter Munk and Bill Bascom in small boats out there to watch these pressure gauges. They had recorders on their boats, little sort of skiffs, so that if anything happened they could observe it right then and there and signal. Horizon stood by, our oceanographic ship; they would signal Horizon; Horizon would

Revelle: send a radio message, and this whole elaborate evacuation thing would get underway.

Of course, there wasn't any landslide. After waiting a suitable length of time, the atomic cloud started coming over in that direction, so Walter and Bill got out of their skiffs onto the Horizon and got out of there. But they then went back later, the next day, to pick up their instruments. On Walter's instrument there was a huge signal [chuckles] after he had left. He's quite certain that if he saw that signal he would have said, "This is a tsunami!" This elaborate evacuation machinery would have been set in motion. He wonders if he would have ever dared to come back after that [laughs]. But fortunately it happened after he'd left the skiff.

Sharp: He might not have been remembered as the darling of the navy, for sure.

Revelle: That's right. So that was an interesting happenstance.

John Isaacs was absolutely overwhelmed by the hydrogen bomb test, which must have been awful. I didn't see it. I'd just come in to Kwajalr, and I saw the flash at Kwajalr. But I didn't get to Bikini until the next day. He wrote a long poem about it which was full of really awful sexual images, he was so overwhelmed by it.

That was the beginning of our Capricorn expedition. Spencer F. Baird and Horizon met up at Bikini and then went for an expedition to the South Pacific. Horizon had been somewhat contaminated by that time, although we didn't realize it. The Baird wasn't, because she didn't come in until after the test had gone off. That was in 1952.

In 1950 we had our Mid-Pacific expedition, and we ended up at Bikini then, too. Russell Raitt made a lot of seismic refraction measurements of the depth of the coral. He found that the surface of the underlying volcanic rock was quite irregular, just as Charles Darwin and J.D. Dana had predicted 110 years before.

Sharp: I started to look at some of the material on Midpac, and I saw that there was some work done at Bikini. I was surprised to see people going back again and again.

Revelle: Oh, yes. People did go back year after year, primarily to study the decay of the radioactivity.

Sharp: I wanted to ask you a couple of questions about the research that went on as a result of the Bikini blast. You were learning quite a bit about an atoll and what its reactions were to the Bikini test and to all the testing. It also seemed like scientists were using the information at Bikini for other sorts of research.

Revelle: We learned several interesting things that were more descriptive than theoretical. Those were the fairly early days of the understanding of diffusion in the ocean. That paper that [Walter] Munk and [Gifford] Ewing and I wrote on diffusion in Bikini Lagoon is one of the first empirical measurements of oceanic diffusion.*

What we learned from the Wigwam test was quite interesting. A man here at Scripps named Ted [Theodore R.] Folsom designed a probe which would measure radioactivity in the water. When we lowered this probe in the area of the Wigwam test, what we would find is that it would go down a certain distance without showing much of a signal, then all of a sudden you'd have a huge signal. Then go down a few more meters and the indicator would drop right back to where it started. You'd find several of these very sharp, sudden signals at different depths. That's where my figure of speech about the ocean being a deck of cards comes from. The layers in the ocean are lenses, actually, of water of a particular temperature and salinity, and therefore a certain density, which sort of sit on top of each other and behave separately.

Sharp: And don't mix?

Revelle: And don't mix. They mix at the edges, apparently. One of the curious results we obtained was that every time we lowered this thing, this probe, we'd get these sudden, sharp breaks, but they weren't at the same depth. Each lowering showed a series of layers at different depths. The picture that I have of them is that they are lenses of water which don't extend indefinitely; they go for a certain distance and then peter out, just sharpened up to a point.##

*"Diffusion in Bikini Lagoon," Transactions, American Geophysical Union 30 (No. 1, Feb. 1949), pp. 59-66.

Revelle: You can see somewhat the same thing if you're watching the ocean after a storm. You'll see that the mud that washes out from shore goes out to a certain distance and then just stops. It's a water mass that is a lens which has a definite boundary to it.

Folsom and Ed Goldberg and John Isaacs and I wrote a paper for the first International Conference on the Peaceful Uses of Atomic Energy, in which we talked about some of these oceanographic results.*

Since then, of course, there's been a tremendous advance in the study of these processes of eddy motion. There are now known to be large eddies--100-200 kilometers in diameter--called mesoscale eddies. The best pictures of those are taken from satellites off major ocean currents like the Gulf Stream and the Kuroshio where large swirls of water break off and move away from the current and persist for months, isolated water bodies some distance from the current.

On the warm side of the current the eddies are relatively cold compared to the water that's there, because the whole stream meanders, and the meanders finally break off into these eddies. On the cold side there are warm eddies; in other words, on the shoreward side the eddies are warmer than the surrounding water. Because they persist for months they're little zoos; they're isolated water masses that have their own fauna and ecology. The poor creatures in them, of course, are eventually lost when the eddies disappear. But they persist, as I say, for a long time.

These are comparable to the cyclones in the atmosphere, not hurricanes, but the big wave motions, eddy motions, in the atmosphere. But quite different in scale; cyclones in the atmosphere persist for about a week, whereas these things persist for five or six months or more. And they're very much smaller in size. The atmospheric cyclones are a thousand miles across; the mesoscale eddies in the ocean are about a hundred miles across.

They have a profound influence on the circulation of the water or on the movement of heat, for example, from low latitudes to high latitudes. But that didn't come out of the atomic bomb tests. That came out of studies of the Gulf Stream and the Kuroshio.

Sharp: The USGS, United States Geological Survey, and Harry S. Ladd are names that show up for a long time.

*"Nuclear Science and Oceanography" (with T.R. Folsom, E.D. Goldberg, and J.D. Isaacs). International Conference on the Peaceful Uses of Atomic Energy. June 30, 1955.

Revelle: And Joshua Tracey is another one.

Sharp: The USGS published this Professional Paper 260 series on the Bikini scientific work. There are two dozen papers if you look at the index. I wondered how widely circulated within the scientific community this particular series of papers was.

Revelle: The Professional Papers of the Survey are very widely disseminated. They're the best of American geology. The Survey has had a long history of doing first-rate science. Harry Ladd had been interested in coral atolls all his life, coral reefs and atolls, and had studied quite a few of them in the Pacific. He was one of the Crossroads oceanographic party, and so was Josh Tracey, but only in the re-survey of Bikini in 1947, when they brought out a drilling rig and drilled into the atoll.

One of the clear outcomes of the Bikini operation scientifically was the fact that Darwin was right.

Sharp: About the coral reefs?

Revelle: About coral atolls. The atolls are formed by the sinking of seamounts, which were originally island volcanoes.

One of the interesting questions that is still not resolved is why some islands become atolls and others become guyots, don't become atolls.

For example, on our Mid-Pacific expedition we found a lot of flat-topped seamounts in the Mid-Pacific mountains, as much as six thousand feet deep, a mile deep or more. These were covered mostly with manganese crusts. In those crusts there were also corals, not very many of them but still enough to clearly identify them as shallow water corals of Cretaceous age, sixty million years old. So sixty million years ago those guyots in the Mid-Pacific mountains were in fact above sea level and were planed off by the waves to a flat-topped surface. That's how they became flat-topped, in fact.

Corals grew on them, but they may have sunk so fast that the coral reefs could never get started, or more likely the surrounding sea water was too cold for reef coral growth, whereas at Bikini and Eniwetok and Kwajale, in those Marshall Islands, the water was warm enough for rapid reef coral growth so that coral reefs could keep up with the sinking or the subsidence and stay right up at the surface.

I think we really understand atolls now pretty well. They always have a lagoon in the middle, which is as much as 180 feet deep. This lagoon originated during the last glacial periods of the Pleistocene when the sea level was much lower than it is now

Revelle: because of the ice locked in the continental ice caps. Many of the atolls were planed off then by waves at about the present depth of the bottom of the lagoon. Then when the sea level rose again, the outer edge of the atoll, the reef built up. But it couldn't build up in the lagoon because there weren't enough nutrients coming into the interior of the atoll.

So coral reefs in general build up around the edge at sea level, and in the center you have this hole. They're sort of like a doughnut. The doughnut is not continuously usually; there are passages between islands. I found a little map of Bikini Lagoon. [opens map] Here it is. This is something Ellen found in some old file. This is what we used to look at the bomb through. [shows interviewer pair of cardboard dark glasses] At that time, on the cover of this envelope, I had drawn a map of Bikini Atoll.

Sharp: So here's Bikini Island.

Revelle: That's the island of Bikini.

Sharp: This is--

Revelle: Enya Island. This is Namu Island here. And then a whole series of islands around this side. This reef here. I was quite surprised to find that envelope yesterday.

Sharp: This should go in the SIO archives for sure.

Revelle: With this thing [the dark glasses].

Sharp: I had seen a note that there had been--.

Revelle: What's this over here? Yes. It even shows where the Crossroads operation took place.

Sharp: And then the target ships--

Revelle: Were here. This distance from here to here is about twenty miles.

Sharp: Now where would Rongelap--?

Revelle: Rongelap was way off here somewhere. The atoll to the east. Eniwetok was right here, to the west. The oceanographers had instruments all through Bikini lagoon.

Sharp: I had seen that too, that the instruments were on different islands, sort of placed all around. So were the people.

Revelle: There was just reef here. This is a very steep reef, very abruptly dropping into deep water. The currents come in this direction so this part of the reef is building up all the time.

- Revelle: But this reef, on the other side of the atoll hasn't had much chance to build up because there isn't as much food for the corals to eat there.
- Sharp: The flow stops, goes that way.
- Revelle: The currents go in this direction, so you can see how the reef built out on the currentward side of Bikini Atoll and off here on the current side of Enyu Island.
- Sharp: In this photojournal that was done by the historian of the operation some of the scientific conclusions were included. The historian said that it took forty-eight days for the exchange of the lagoon water with--.
- Revelle: The water outside. That was one of our oceanographic results. Bill Von Arx and Bill Ford studied the currents in Bikini Lagoon and the exchange with the outside water. That was one of their conclusions.
- Sharp: It would have meant, then, that the radioactivity--
- Revelle: That was in the lagoon stayed there pretty well. It decayed a long way before it got out. The radioactivity outside was from the cloud, from the fallout.
- Sharp: The airburst.
- Revelle: And the waterburst.
- Sharp: And the waterburst once the water became the mist.
- Revelle: There was a big cloud above the waterburst, too; a lot of stuff went up into the upper air. We had a whole fleet of ships outside the atoll to follow that radioactivity. John Lyman was in charge of that. We had, I think, something like ten or twelve ships out there.
- Sharp: The numbers of people who were there in some capacity was over five, six, seven thousand people.
- Revelle: In the task force, you mean.
- Sharp: Right. Everybody.
- Revelle: Is that all? I should think it was more than that.
- Sharp: Well, the historian said ten thousand. That seems so fantastic that I didn't believe it.

Revelle: I think it's at least that number. There were dozens of ships there besides the target ships. There were destroyers, minesweepers, transports, scientific ships--scientific in the sense that there were scientists on them--patrol craft of many different kinds, landing craft. Some of these ships were quite big, like the Fulton, which was an old submarine tender, a huge ship. The Albemarle--they called it the Able Mable--that was a big ship. The ship that I was on, as I remember, the Kenneth Whiting was middle sized.

Sharp: The only other question I had about this diffusion article, and you pretty much outlined what you thought was important about it, but were you using fairly classical scientific methods to do the measurements and the testing or were there new methods developed to do the particular measuring and observing at Bikini?

Revelle: No, I don't think so. It depends what you're talking about. Are you talking about the diffusion measurements?

Sharp: Yes.

Revelle: Those were quite straightforward. You simply took samples of water and measured the radioactivity in them. Of course, the new thing about that was measuring radioactivity. Never been done before. But the water samples were just plain water samples. It wasn't until later that Ted Folsom developed his probes.

There were a lot of new instruments developed during World War II, particularly recording echosounders and towed magnetometers. The great development in electronics made it possible to have very good recording of measurements, and continuous recording. One of the important things that came out of World War II, I'm pretty sure, was something very simple called an O-ring. This is basically just a rubber doughnut which you put between two parts of a steel case, inside of which you put your instruments. Up until that time it had been very hard to use electronics below the surface in the water because the darn things would leak.

The interesting thing about the O-ring was that--supposing we have a cylinder here like this and then another part of the cylinder here. You put the O-ring right around here in a groove. As the thing gets deeper, the pressure builds up; these two sides squeeze together and the O-ring is compressed and squeezed in such a way that it makes a better and better seal the more pressure there is. You can go down to very great depths with these things and not have any leakage at all.

Sharp: So they still work properly.

Revelle: The pressure helps them work; the pressure is working for you instead of against you. Instead of the water squeezing in between

Revelle: the two parts of the cylinder, this O-ring squeezes out. That was a very simple but very important development.

In fact, in general I would say that the progress of oceanography depends very largely on new technology. Every time we get a new technology we find out a lot more about the ocean.

Sharp: Regarding the impact of the testing at Bikini on the navy, you touched briefly on the idea that there was some real disappointment because the damage to the target ships was so great that it was thought that navy ships were obsolete. Any other sorts of effects on the navy and their ideas about possibly using atomic weapons at that point?

Revelle: As I say, the main tactical or strategic lesson that was learned was that you had to keep your ships fairly far apart so you couldn't destroy more than one at a time. That meant somewhat of a change in naval tactics and more action at a distance, as I said. The one thing that they had not expected, I think, was the radioactive contamination.

Sharp: And what to do about it.

Revelle: Not much to do about it, it turned out.

Sharp: Who did they think was going to use these atomic weapons? Did they assume that Russian development of atomic weapons was such that there was an immediate threat?

Revelle: No. The Russians didn't have any atomic bombs at that time, and there were some people out at Bikini, Admiral Solberg was one, who suggested that--[brief discussion about a whale visible off the coast]--that we should tell the Russians that we would use our atomic weapons against them if they developed them. That we ought to decide right now to abolish the things on both sides. There was one famous Baruch plan, Bernard Baruch and [David E.] Lilienthal, I guess, who was by that time the chairman of the Atomic Energy Commission, which had tried to reach an agreement with the Russians about atomic weapons, to not develop them, not have them. But this never got anywhere.

At the same time, because of the work of Szilard and others in the Federation of Atomic Scientists, there was a famous legislative act passed [in August 1946] which made the Atomic Energy Commission a civilian agency, under civilian control and not under military control. All of the subsequent development was through the Atomic Energy Commission, which was quite independent of the armed services. There was a Senator, who was one of the chief authors of that act. His name was MacMahon.

Revelle: The Atomic Energy Commission, particularly under Lilienthal's guidance, was very stuffy about the military, keeping the military more or less at arm's length. They were also quite--well, they would lie about the effects of radioactivity.

Sharp: To minimize them?

Revelle: Yes, to minimize them. It was really very bad business the way those guys in the AEC behaved. You may have read about these suits in Nevada and Utah. They just refused to admit that they had done any damage. Finally, of course, the pressure built up to the point in the [John F.] Kennedy administration where atmospheric testing was completely banned; only underground testing could be conducted. That's still the case. That, of course, was the end of all testing out in the Pacific.

One of the events that affected that a lot was the radioactive contamination of a Japanese fishing ship, the Lucky Dragon as a result of these tests. It was badly contaminated, and I think several people were killed--I'm not quite sure about that--by the radioactive contamination. In the United States there was increasing amounts of Strontium 90 found in children's milk and things like that.

Bill Libby, who was an Atomic Energy Commissioner at that time, spent a lot of time studying the distribution of the radioactivity. It became very widespread in the United States, and the movement against the airborne testing was led by Linus Pauling.

Sharp: Did you get involved in any of that kind of opposition?

Revelle: I was very unhappy about the AEC people because as I say they would lie about the extent of the radioactive contamination. But I never got involved with the Federation of American Scientists.

Sharp: Was that a conscious decision not to get involved?

Revelle: Yes, it was, not because we were beholden to the Atomic Energy Commission, quite the other way. But because of my job as director of Scripps I felt that I should be as apolitical as possible. I never got involved with any of these ban-the-bomb movements, although I'm thoroughly in favor of banning the bomb.

I was at Los Alamos frequently after the war, during the first five or six years after the end of the war. I learned quite a bit about their technical developments. They were a remarkable group of people in terms of the skill of their operations and the effectiveness of their work in conducting big tests like this. Most of the people in the top government scientific hierarchy these days are Los Alamos people, or at least

Revelle: Edward Teller people, which I don't think is a good thing, but anyhow that's the way it is.

Sharp: You mean because of the supportive attitude?

Revelle: Yes.##

In the Federation of American Scientists is one of our people at La Jolla, Herb [Herbert F.] York, who was at one time Deputy Secretary of Defense and Director of Defense Research and Engineering, DDR and E. Before that he had been director of the Livermore laboratory. He was right in the middle of the development of the hydrogen bomb; it was largely done at Livermore. He's now one of the leading doves of the country, as are most of these early people in the atomic bomb business. It's amazing how those who know most about it are most against atomic weapons.

Sharp: It should tell you something, because they've taken the turn they have, because of what they know about the effects of it and the destructive possibilities. Did it bother you that you felt unable to be more supportive of the opposition? You had the responsibility for being director of Scripps, and you said that you didn't think it was a good idea to be publicly connected with the opposition.

Revelle: Well, both ways. It wasn't a good idea to be politically involved on either side.

Sharp: Does it fit well with the way you think about things anyway, or does that bother you that you couldn't be more involved politically one way or the other?

Revelle: I've been in the Pugwash movement from very close to the beginning of the Pugwash movement. I guess what I feel there is something a little bit different, that the intellectual part of this nuclear weapons business is a very complicated chess game, such things as counterforce strategies, for example, and counter-counterforce strategies. It goes on and on. You have to be deeply immersed in it to be very effective or have very useful ideas. I just never learned enough about it to feel that I was competent to do much in the field.

For example, Alvin Weinberg has recently written a paper which he called "The Defense Protected Build-down." He was director of the Oak Ridge National Laboratory for many years. The chess game type of thinking that goes into that is something that you can't really contribute to unless you've been, as I say, thinking about it more than anything else, as Herb York does, for example.

Revelle: so in our little program here on Science, Technology, and Public Affairs, the aspect of arms control is the one that he is completely involved with. I'm involved more with the problems of developing countries. All I do as far as atomic weapons are concerned is just try to learn from Herb, from his colleagues, but not really trying to contribute to it intellectually.

I guess my natural inclination is to say that the role of the scientist is to try to think through the problem rather than to try to take a political stand and to try to state the facts as best he can.

If you scratched me and asked me my own personal opinion, it is that I'm in favor of unilateral disarmament. I think it's ridiculous to try to keep up with the Russians.

I'm thoroughly in sympathy with Bertrand Russell's statement that it's better to be Red than dead, even though it's pretty awful to be Red, to be a part of that tyrannical system.

But what we're risking is Western civilization. We're all dead sooner or later, there's no problem about that, but the problem is not to kill our civilization. It's the only one that's ever given mankind hope for the future. So Herb York says I'm right in principle, but politically I don't make any sense.

Sharp: Those of us who are somewhat younger have a sense that we feel the nuclear disarmament question more keenly because we think we're going to be here longer than perhaps the older generation. For older persons the question of nuclear holocaust is slightly more remote, perhaps, than to those of us who are younger and might acutally be around to experience it. I guess I wonder if you really think that, in your lifetime, there really is a possibility of nuclear blasts occurring.

Revelle: Oh, I think so, yes. Certainly the problem with nuclear weapons is not to get killed but to not get killed. The survivors are the ones who are going to be really in a terrible situation, much worse than the ones who are killed.

Sharp: Yes. I think that's the scary part.

Revelle: So that's what scares me, more or less personally.

But what I'm really scared about is not anything happening personally, but as I said, the destruction of what has been built up over the last thousand years in Europe and North America in terms of Western civilization. I've spent a lot of time in Asia and some time in Africa, a little time in South America. I just hate to see the world left in the hands of those people, because

Revelle: they have a very much less hopeful attitude towards the future of mankind. This may sound ironic when we're the ones who created the bomb which has jeopardized our future. But we're also the people who think that it's possible for man to understand nature and understand himself. Nobody else has ever thought that.

The most important thing that has ever happened in the history of mankind was started by the Greeks, namely the possibility of understanding. If we lose that we've really lost for a long time to come. That just gives me the horrors to think of that. Just think about the civilization that created Mozart, that incredible man. Nothing like him has ever happened elsewhere in the history of the world. Or Bach, Beethoven, and Brahms, or Isaac Newton and Charles Darwin, let alone Michaelangelo and Picasso. We shouldn't belittle ourselves; we have an incredible history of changing the fate of man.

Sharp: And sense of beauty.

Revelle: Yes. I admire some Indian art, particularly their carvings, because they're so full of life and so full of joy. And the Mogul paintings; they're also full of life. The Chinese have done wonderful things in art too. But it's only the West which has believed that things could be changed and has taken actions to change them.

The Russians are, in a very real sense, part of Western civilization. It's not as if we were fighting a different civilization. So I'm filled with horror at President Reagan. I belong to something called the Bohemian Club up in San Francisco, and we have an encampment up on the Russian River, a place called the Grove, the Bohemian Grove, which I go to in the summertime. That's full of people who think President Reagan is just great. In fact, they're a little bit disappointed in him because he isn't conservative enough. There are fortunately enough people there who don't think so that I manage not to be too unhappy. You'd just be surprised how many Californian upper-class people, rich people, think that he's just right.

Bill McGill, who was chancellor here at UCSD after John Galbraith and before Bill McElroy, says that Reagan is the toughest politician he has ever known. He's a very good politician but also very tough, very ruthless, really.

Sharp: Very thorough.

Revelle: What do you mean by that?

Sharp: If there's an effort that he wants to accomplish, he has the kinds of people around him and the kinds of ideas, who provide a lot



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Sharp: of follow-through in terms of talking to the right people on the right committees, and so on to get the action put into place.

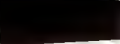
He learned that in being governor of California in some of the policies that he wanted implemented. He has refined it to a very fine degree in his presidential policies. It was a very clear learning process that Mr. Reagan went through and is still refining, and now in Washington he has even more support at his command to put the policies in place, whether they're social policies or defense policies. So that increases his power. I still keep hoping that maybe he really won't run for re-election.

Revelle: He's obviously going to though.

Sharp: Well, I think it's going to be a lot harder. I think some of the Democratic candidates are doing rather well, so I think it'll be a harder campaign.

Revelle: Yes, it will be of course a lot harder. But he seems to be fairly committed to it. Now if you talk about older people, I'm almost seventy-five, and I just shudder to think of having to live the next four years with this bastard as president. I'll be senile before he gets out of office. Maybe not, but it's an awful long period to think about.

Well, we have a lot more to talk about some time.##



VI A GLANCE AT LATER ACTIVITIES

Sharp: I don't know if you remember looking at this sheet, but it's a brief chronology of some of your institutional affiliations, committee memberships and associations. Originally you had talked about SCOR and IOC as two of the most important, and the work in UNESCO I had thought too.

Revelle: Well not so much UNESCO but with the IOC. Let me just give you a list of things that I was involved with starting. One was the SCOR, the Scientific Committee in Oceanic Research. Another one was the Intergovernmental Oceanographic Commission. A third was the International Foundation for Science. The fourth, of course, was the University of California, San Diego. And the Office of Naval Research, I was involved with that too, starting that. So I'm pretty good as a starter of things. I'm not very good at organizing them after they get started.

I was involved with the first International Oceanographic Congress. I had something to do with changing the character of IAPSO, the International Association of Physical Sciences of the Ocean. I was its president at the time it changed its name and its character. I was involved with the Committee on Climate Changes in the Ocean, starting that and getting it under way, which was the oceanographic part of the World Climate Research Program.

I started the Harvard Center for Population Studies and started the Board on Science and Technology for International Development for the National Academy of Sciences, called BOSTID.

Revelle: Among other important things that I've been involved with, one was the so-called Revelle report about agricultural development in the Indus Plain of West Pakistan; the Education Commission of the Government of India; the development of the Scripps Institution as an international institution, world-ranging institution as opposed to a local, California institution; and starting the Institute of Marine Resources.

I think the 1950s were one of the great ages of exploration, in this case the exploration of the bottom of the sea. Scripps played a major part in that, although by no means the only part in that.

Sharp: Midpac was--

Revelle: Was fundamental to our starting, that's right. We perhaps got more results per unit of time on Midpac than any other expedition. The other thing I started was the measurements of carbon dioxide in the atmosphere which began the modern effort on the carbon dioxide problem. I was very much involved with that.

It's surprising to me that in spite of the fact I'm famous as a poor administrator, that my principal contribution to science has been in organizing things.

Sharp: In setting up the frameworks for this kind of work to go on.

Revelle: I learned an awful lot, of course--the basis for all of that was the wartime experience, where I learned how to persuade people and to work with people, and to get them to think that what they were doing was important, what we were doing was important.

Sharp: And persuading them to put financial support behind it, to keep it going.

Revelle: I've never been good at raising private funds, but I've been pretty good at raising public funds.

Sharp: I think we could do well to cover a variety of these topics in our sessions. The papers, the way you're sending them up to the archives fits in very well with these discussions, so it will be just a matter of my going through the right ones and selecting what seem to me to be important issues and questions that you had to deal with, then sending you a sample of them ahead of time to help get your mind refreshed so you can have some recollections, and have a chance to think about them too. That seems to be a workable way for you.

Revelle: Sure. It's interesting; these documents are always fascinating because you forget so much that's in them.

Revelle: But I find this a valuable thing for me because it does get me to be a little bit introspective, which I've never been very much.

Sharp: Your time has been taken up with more present, pressing issues than introspection and considering the past and the things that you've done or not done.

Revelle: Or not done--mostly not done.

Sharp: As we go along I'll keep pressing you to answer my questions, but I hope that we're incorporating the issues that you think are important. That's the whole purpose really. I come at it very much as an outsider and an observer.

Revelle: I can see from your list that you simply looked at the sort of formal record rather than what really went on. For example, the Naval Research Advisory Committee, I never really had--I was a member of it for ten years, but it was a completely useless committee, I thought at least.

One of the things I feel I did do was create a lot of oceanographic leaders. That's what Scripps is most famous for. Many directors of other institutions got their Ph.D. at Scripps.

Sharp: So training them and I would think to be somewhat entrepreneurial and going out with their skills--.

Revelle: Exactly. Very much so. Entrepreneurial is a good word. John A. Knauss is one; Art Maxwell is another; Ross Heath is still a third. Don Pritchard is still another.

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